SCIENCE

19 May 1961

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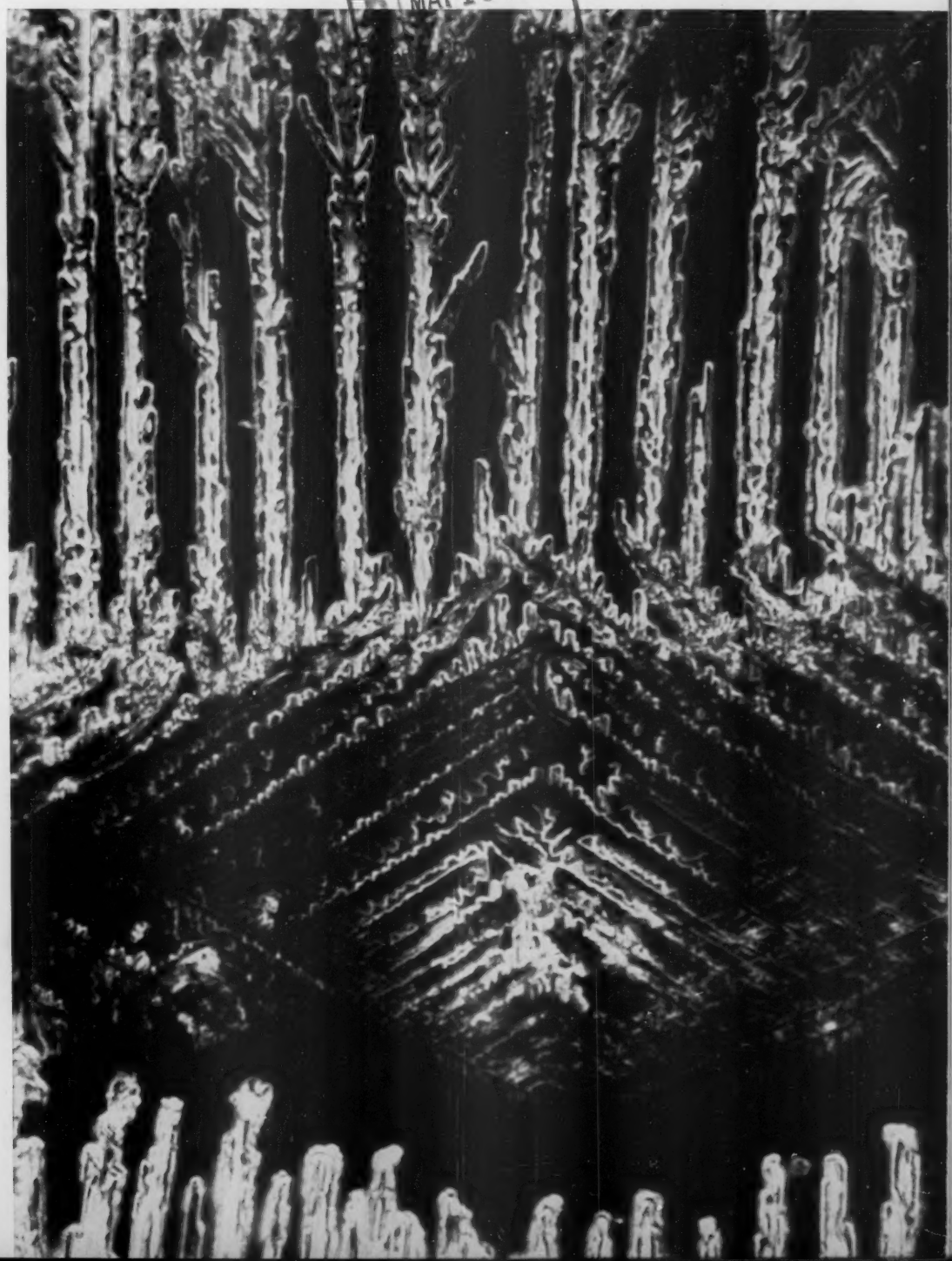
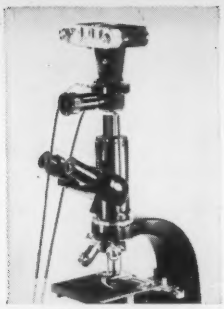
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





For photomicrography and simultaneous observation, binocular photo tube "FS" is available

the laboratory microscope which fulfills research requirements

Model S 48/76

the new LEITZ LABOLUX IIIa

The Leitz Labolux IIIa is a new laboratory microscope with built-in illumination and the famous Labolux ball-bearing focusing control, which combines both coarse and fine focusing in a single knob. Available with a wide variety of accessories, the Labolux IIIa is recommended for all routine laboratory work and, in addition, can be equipped to fulfill the most exacting research.

A variety of interchangeable tubes is available: monocular, binocular or trinocular (binocular viewing plus photo tube for photomicrography). Tubes can be rotated 360° so that the Labolux IIIa may be faced away from the observer, for increased accessibility to all controls and to the object stage, and to make "conference-viewing" by two consultants more convenient.

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LABOLUX IIIa, Model S 25/95

• inclined binocular tube S with knurled knob to adjust for proper interpupillary distance • built-in mechanical stage #25 • two-lens condenser #95 • substage unit with rack and pinion focusing accepts sleeve-type condensers • quadruple nosepiece with achromats 3.5X, 10X, 45X and 100X oil immersion, the last two having spring-loaded mounts • paired 10X wide-field eyepieces • horizontal carrying case • 3-step transformer, 6 V., 2.5 Amp.

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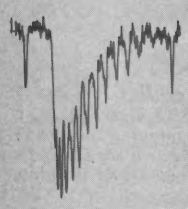
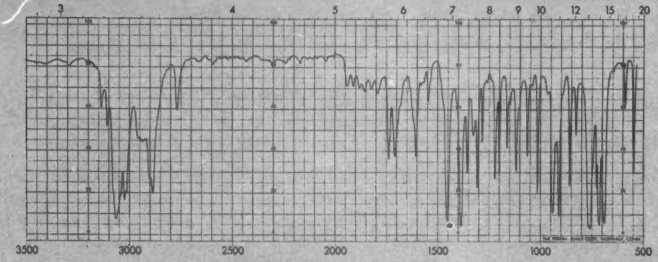
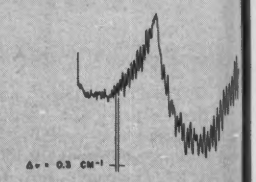


NH_3 between 970 and 920 wavenumbers.



Indene from 3500 to 530 wavenumbers.



CS_2 between 2200 and 2165 wavenumbe

NEW MODEL 421 HIGH-RESOLUTION SPECTROPHOTOMETER OFFERS UNINTERRUPTED SCAN FROM 2.5 TO 18 MICRONS

Highest grating resolution and greatest flexibility over an extended wavelength range—with no gaps or overlaps—the new Perkin-Elmer Model 421's performance means these advantages:

More accurate identifications: significant absorption bands differing only slightly in frequency can be easily separated by the outstanding resolution of the Model 421's grating dispersion system.

Uninterrupted scan: automatic filter switching as scan proceeds does not interrupt spectral record: the Model 421 presents a continuous, complete grating scan.

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All the instrumental operating features of other Perkin-Elmer spectrophotometers—double-beam optical null principle, speed suppression, automatic gain control, scale expansion and compression—are included in this new instrument. A full line of sampling accessories is offered. Write for complete information and sample spectra.

INSTRUMENT DIVISION
Perkin-Elmer *Corporation*
NORWALK, CONNECTICUT

19 May 1961, Volume 133, Number 3464

SCIENCE

3 of the 33 NEW ITEMS

featured in our new issue of

WHAT'S NEW FOR THE LABORATORY

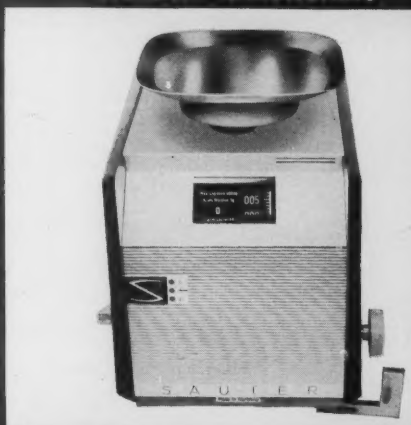
Ask for No. 43!

It's easy to separate, detect and identify the components of a syringe-injected sample with this new "Precision" instrument—designed for ambient temperature work. The *Chronofrac* can be set up ready for use in ten minutes. Practically any gases and volatile liquids with boiling points to 105°C can be analyzed. Either air or helium may be used as the carrier. *Details are given in bulletin 619, sent on request.*

C-4280X "Precision" Chronofrac, 11"x13"x5" high $250.00

"Precision" CHRONOFRAC

This *Toppan* scale has a unique full-floating indicating mechanism for fast, correct weighing—regardless of off-level conditions. Simply place it on a table, put the sample on the pan, and read the weight. For weighing beyond the optical range, just remove a substitution weight. The added capacity is indicated in the aperture adjacent to the projection scale. Features include front and rear direct-projection reading . . . magnetic damping for quick oscillation and stability of projection scale . . . extra-large interchangeable pans. There are six models (three with tare, three without) in three capacities—1000, 2000 and 4000g. Prices range from $580.00 to $730.00. *Write for descriptive brochure.*



Sauter TOPPAN SCALE

Even the minutest change in moisture is detected by this new portable unit which monitors water vapor continuously in ppm ranges of 0-10, 0-30, 0-100, 0-300 and 0-1000—selected by a 6-position switch. Accuracy is ±5% of the full scale in any of the ranges. Response is rapid. Reading is direct. The sample flow rate is 100cc per minute, adjustable for any gas stream. Sample pressures are 10 to 100 psig. The unit plugs into any 100-125 volt AC outlet—or it may be operated from the self-contained 67½-volt battery. Comes complete with electrolytic cell. *For the entire story, ask for bulletin 773.*

H-9560X Beckman Hygromite, 11" wide x 6.7" high x 10.7" deep $495.00

Beckman HYGROMITE

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60 kV
30,000X
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- Microscope may be tilted for stereo-photography
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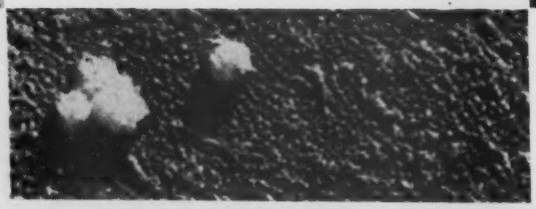
- Uses standard 35 mm film (36 pictures) or ten 2" x 2" plates
- Dimensions of basic instrument: 14½" wide, 18" deep, 34½" high
- Low initial cost and minimal maintenance



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NEW TORSION DIAL BALANCES with WEIGHT LOADERS

Torsion's new line of weight-loading dial balances retains all the proved advantages of the unique Torsion principle which eliminates knife edges and guarantees long-lasting accuracy.

Since Torsion introduced the "fine weighing" dial over a year ago, users have reported substantial savings in weighing time. Now Torsion has added a "weight-loading" dial which enables the user to "dial in" additional weights as described in the specifications for each new balance.

Both dials can be used without arresting the balance.

By using two dials, one for weight loading and one for fine weighing, Torsion has cut weighing time even more.

With Torsion's new two-dial feature, the time-consuming handling of small, loose weights has been eliminated. In addition to faster weighing, Torsion's new dial balances with weight loaders minimize the possibility of weights becoming inaccurate from rough handling.

Ask your laboratory supply salesman for a demonstration or write for complete specifications.

The **Torsion Balance** *Company*

Main Office and Factory: Clifton, New Jersey
Sales Offices: Chi., Ill., San Mateo, Cal.

A TORSION MODEL DWL-3
Capacity: 200 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .02 gram graduations
(Readability: .005 g)

B TORSION DWL-5
Capacity: 500 grams
Weight-loading Dial: up to 90 grams by 10 gram increments
Fine Weighing Dial: 10 grams by .1 gram graduations
(Readability: .02 g)

C TORSION DWL-2
Capacity: 120 grams
Weight-loading Dial: up to 9 grams by 1 gram increments
Fine Weighing Dial: 1 gram by .01 gram graduations
(Readability: .002 g)

TORSION DWL2-1
Specifications are same as the DWL-2 except that this model has scoop for seeds or other bulky material.

The LGP-30 Electronic Computer is designed to stop the juggling of figures —and start the creating of profits.

The Royal Precision LGP-30 can solve routine and theoretical mathematical problems 30 times faster than any man—yet rents for little more than the salary of an additional engineer.

It is simple to program and operate, so no special programmer is needed. An engineer can use it himself. It requires no air-conditioning or costly installation. It plugs into any 110-volt AC wall outlet. It is mobile, so it rolls anywhere. It is desk-size, so it requires little room.

This means that the Royal Precision LGP-30 is ready to go to work to help your company create new products—and fresh profits—the very same day it is delivered. Can you wonder that there are more LGP-30's installed and working right this minute than any other electronic computing system in its class?

For more information: write Floyd Ritchie, Royal McBee Corporation, Port Chester, New York.

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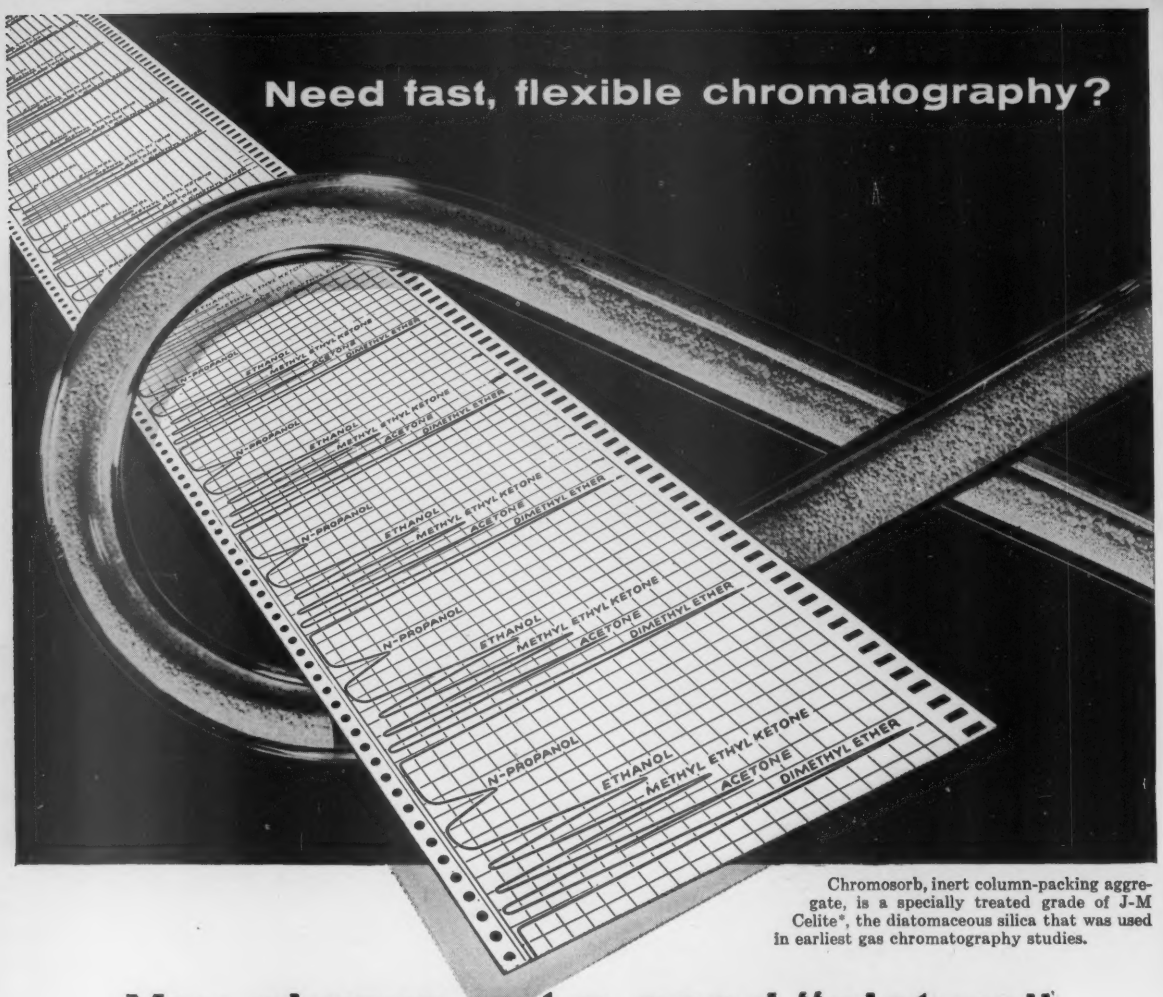

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You always get a good "picture" with Johns-Manville Chromosorb

As an aggregate in gas-liquid partition chromatography, J-M Chromosorb® gives high partitioning effect with maximum number of theoretical plates. Good resolution is obtained because it is chemically inert and won't adsorb components being passed through. You get uniform results, optimum reproducibility, and good flow of carrier gas without excessive pressure drop.

Chromosorb combines optimum surface area with high uniformity. For reduction of fines, all grades are water-screened to close tolerances. Packing is easy. Its physical stability and non-adsorption let you re-use the same column packing again and again. For further information, contact the dealer nearest you.

*Celite is Johns-Manville's registered trade mark for its diatomaceous silica products.

For gas phase chromatography or where inertness is needed . . . Chromosorb

Typical Properties

Calcined diatomaceous earth aggregate.

Color	light pink
Free Fall Density—lbs./cu. ft. (avg.)	20-23
Specific Gravity—true	2.15
Water Absorption—cc./gr. (avg.)	2.4
Moisture—% by weight, maximum	1.0
pH (avg.)	6-7
Surface Area—sq. m./gm. (avg.)	3-5

For chromatographic studies . . . Chromosorb W

Typical Properties

Flux calcined diatomaceous earth aggregate.

Color	white
Free Fall Density—lbs./cu. ft. (avg.)	15-16
Specific Gravity—true	2.30
Water Absorption—cc./gr. (avg.)	4.0-5.0
Moisture—% by weight, maximum	1.0
pH (avg.)	8-10
Surface Area (BET Method)—sq. m./gr. (avg.)	3-4

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** The Picker Nuclear Division is prepared to help interested institutions in drawing up training programs in the use of nuclear techniques in biology, chemistry, medicine, agriculture, physics, and other fields.*

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Standing behind every Picker instrument is a local member of the Picker X-Ray national sales and service network.
He's there to protect your investment.
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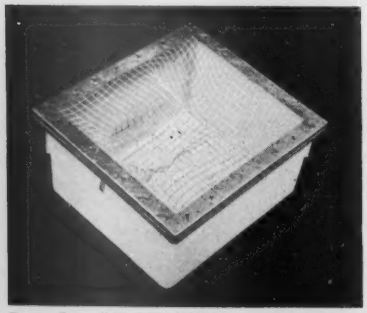
A Mousellany of Animal Care Developments

New Standard Of Living For 750,000 Hamsters

Last year more than 750,000 hamsters were used in research. Following about 20 million mice, 10 million rats, and 800,000 guinea pigs used annually, hamsters are now the fourth most widely used experimental animal. Hamsters are increasingly important in infectious and parasitic disease investigations, vascular studies, nutrition and dental research, the study of estrogenically active compounds, and in cancer research.

The Institute of Laboratory Animal Resources, an agency of the NAS, as part of its research animal standards program, has just published "Standards for the Breeding, Care and Management of Syrian Hamsters." These Standards are designed to minimize biological variability by suggesting methods of standardizing and controlling genetic and environmental factors regarding hamsters.

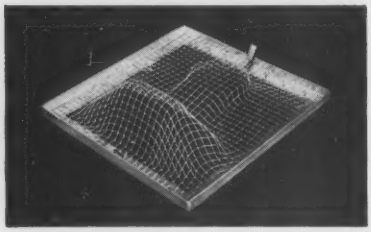
The Standards, prepared by a committee of animal care experts, make recommendations regarding construction and air conditioning of facilities, and regarding such equipment as cages, racks, food hoppers, water bottles, and mechanical cage washers. The Standards also cover food and bedding, cleaning techniques, refuse removal, disease and parasite control, and record keeping. *Copies of the Hamster Standards are available on request from the Executive Secretary, Institute of Laboratory Animal Resources, National Academy of Sciences, Washington 25, D.C.*



Econo-Cage #55, LID #52ES

Hamster Housing That Meets The New Standards

Now, the new "50 Series" of cages and lids designed especially for hamsters is available from the Econo-Cage Division of Maryland Plastics, Inc. This series complies in all respects with the new ILAR Hamster Standards, just as the "20" and "30 Series" more than meet ILAR mouse standards and the "40 Series," ILAR Rat Standards.



Econo-Cage Lid #52ES

The hamster standards recommend a minimum cage area of 150 square inches per nursing female including litter, or a minimum of 15 square inches for each animal over 3 months old, and a depth of not less than six inches. The "50 Series" Econo-Cages *exceed* these specifications in all dimensions. They are 12″ by 14″ by 6½″ deep (i.d.) and 12⅞″ by 14⅞″ by 6⅝″ deep (o.d.), a floor area of 168 square inches... room for 11 adult hamsters per cage.

In further accordance with the standards, the cages are molded with a smooth seamless finish and sides, bottoms and corners are slightly rounded so that there are no dirt-catching crevices. Because they are impervious to liquids and moisture, "50 Series" Econo-Cages are far more corrosion resistant than galvanized pans (and they are far less expensive than stainless).

Polypropylene Econo-Cage #55 can be exposed to repeated cage washings—even to repeated autoclaving—without distortion. Its impact resistance is excellent. The clear hamster unit, Econo-Cage #53 made of acrylonitrile-styrene copolymer, affords constant and immediate visual access at reasonable cost. This unit should not be exposed to temperatures above 200°F, but can take repeated washings at that temperature.

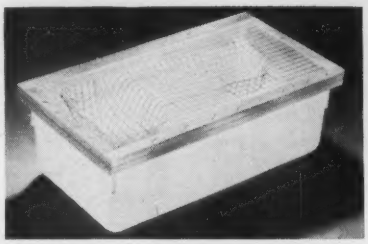
Clamping The Lid On Animal Housing

Newly designed lids, with built-in food hoppers and water bottle pockets, *have special spring steel hold-down clamps which securely fasten the lid to the cage.* The cages are escape proof as the hamsters cannot move the lids from the inside. The lid can be easily snapped into place or removed by the operator.

Lids for the "50 Series" of Econo-Cages are made entirely of metal. The new cover, 52ES, a single piece of #2 heavy gauge galvanized mesh, is deep drawn to form a feeder and water bottle pocket, and riveted to a heavy galvanized frame.

The deep feeder is designed so that animals can obtain food from the entire surface. The food hopper complies with the standards which recommend at least 1″ of clearance between food hopper and bedding. The water bottle pocket, a new built-in Econo-Cage feature, holds most bottle styles and sizes, saves space too.

Lids are also available with a flat mesh top for use with internal feeding systems. Either deep drawn feeder or flat style lids, can be made with or without hold down clamps, deep drawn feeder with spring clips (52ES), deep drawn feeder without spring clips (52E), Flat Lid with spring clips (52FS), flat lid without spring clips (52F). All four lid styles are interchangeable with "50 Series" Cages.

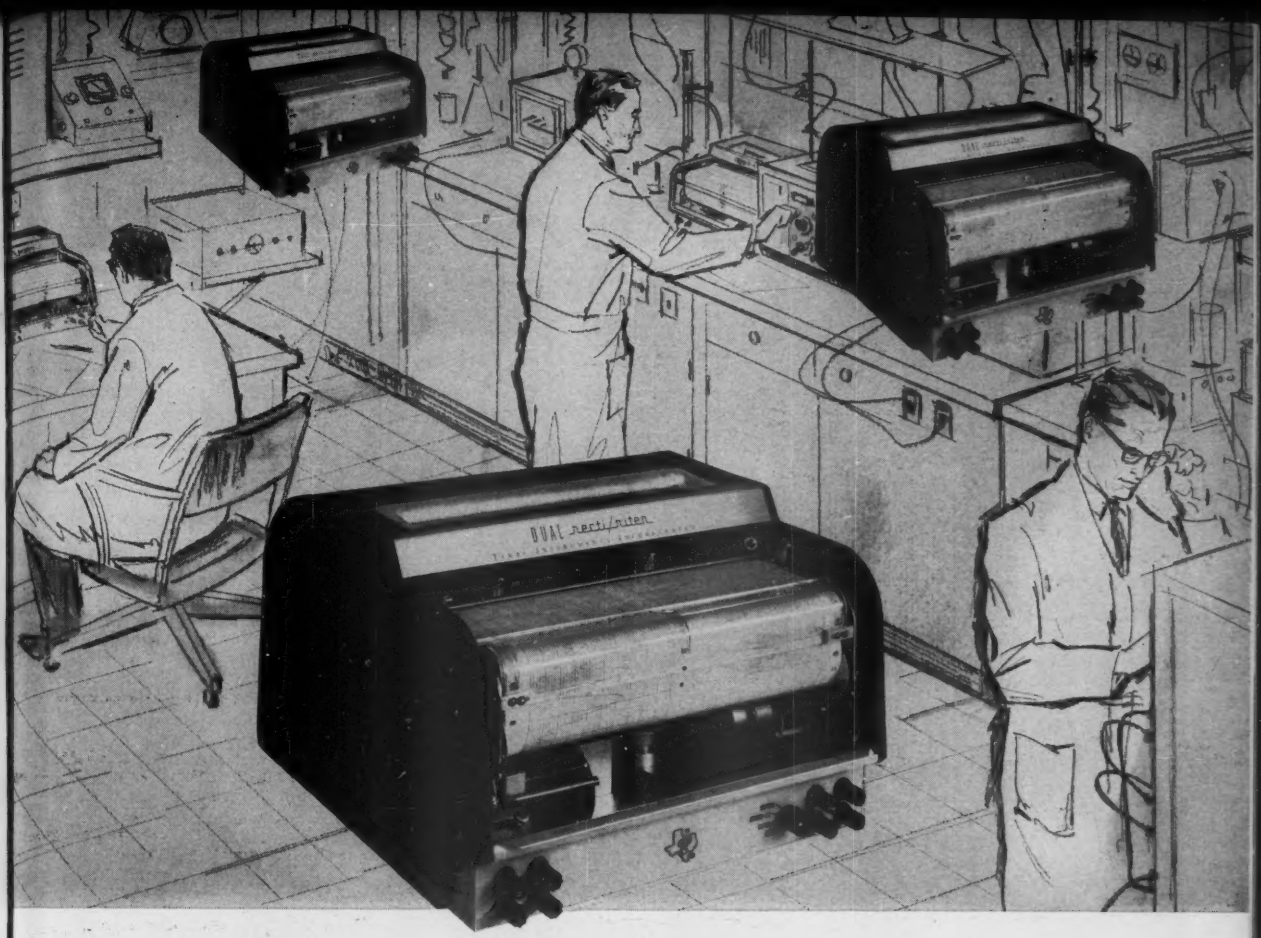


Econo-Cage #45, LID #42E

A Note About Versatility

New "50 Series" cages and lids not only meet ILAR Hamster Standards, but comply in all respects as well with standards established for the breeding and housing of laboratory rats. Conversely, "40 Series" Econo-Cages (10½″ by 19″ by 6⅛″ deep), originally designed for rats, meet the hamster standards. "40" or "50 Series" Econo-Cages now can be used interchangeably for hamsters and/or rats.

For more details on Econo-Cage line write for complete information, including "30 Series" breeding and holding cages for mice (10½″ by 19″ by 5⅛″) available in fibreglass as well as in the same materials as the "40" and "50 Series."



operator convenience makes the recti/riter® recorder preferred for laboratory use

The *recti/riter* recorder has become the *accepted* laboratory recorder—is preferred for the exacting tasks of laboratory applications. The portable *recti/riter* is the only galvanometric rectilinear recorder designed specifically as a bench-top instrument with all routine controls and adjustments located up front for extra convenience. The "writing desk" chart carriage permits operators to make the extensive notations usually associated with laboratory use while the instrument is recording.

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Two-Cycle Pen Response

d-c Milliampere Ranges	½ ma to 100 ma
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a-c and d-c Voltage Ranges	10 V to 1000 V
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Special options and accessories further expand the versatility of *recti/riter* recorders. Write now for complete information on this *accepted* laboratory recorder line.

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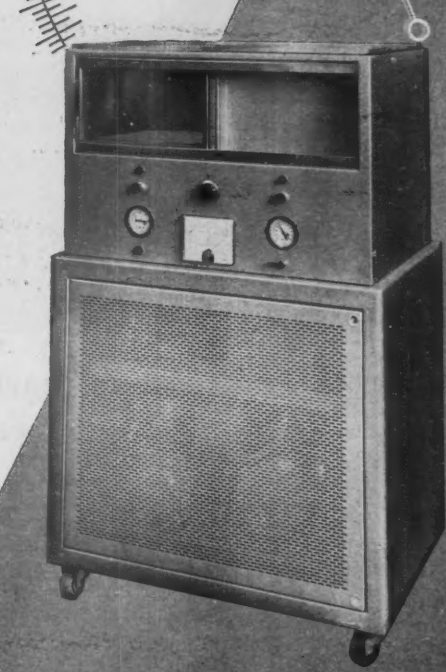
The increasingly vital role of freeze drying in the preservation and concentration of critical substances demands better engineered, more versatile apparatus. Now Amsco Del-Vac Freeze Dry Apparatus fulfills this need within laboratories, pilot plants and production facilities.

Significantly finer performance and versatility of Amsco Del-Vac Freeze Drying equipment permit the direct transfer of process data from laboratory or pilot models to large scale production models . . . without intermediate stages of pilot operation.

Whatever your freeze dry needs . . . whether you require heated, refrigerated process shelves; hydraulic stoppering shelves; a powerful, built-in 5 to 10 micron vaccum pump; or fully automatic controls . . . Amsco Del-Vac has an appropriate model.

Write for fully illustrated brochure IC-606.



Most advanced pilot and light production freeze dryer available . . . Amsco Del-Vac Model 22424RHVM, with heated and refrigerated process shelves, hydraulic stoppering shelves, 5 to 10 micron vacuum pump . . . The ultimate in freeze dry efficiency.

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The New Brunswick Scientific Company Presents The PsycroTherm(TM)

A CONTROLLED-ENVIRONMENT INCUBATOR-SHAKER

Patent Pending

PsycroTherm can be used as an Incubator, Shaker, Environmental Chamber, BOD Incubator, Refrigerator

For the Growth of Microorganisms under Controlled Conditions of Temperature, Atmosphere, and Agitation

THE PSYCROTHERM is a rigidly controlled environmental incubator with a continuous-duty shaking mechanism. Though it occupies comparatively little floor area, it has 10½ cubic feet of *usable* work space in the incubation chamber, where static and shake cultures can be incubated simultaneously or separately.

A VERSATILE UNIT

With fully integrated heating and refrigeration systems the unit is ideal for work with psycrophilic, mesophilic, and thermophilic systems. Temperatures can be accurately regulated from 0° C to 60° C with a control tolerance and temperature gradient both within ± 0.5° C. In non-refrigerated units, the temperature range is from ambient to 60° C, with the same tolerance and gradient as above.

There are many interchangeable shaker platforms. They have large capacities for flasks, tubes, and other culture and reaction vessels.

CHOICE OF SHAKER MECHANISMS

The degree of agitation can be selected and the temperature controlled for the growth of aerobic and anaerobic organisms. Models are available with either Gyrotory® or reciprocal agitation, and illumination for photosynthesis studies. The PSYCROTHERM can also be used as a BOD incubator.

CONTINUOUS DUTY SHAKING

The shaking mechanisms are precision built for continuous operation, long life, and for smooth, quiet, reproducible agitation. Speed is adjusted mechanically and will never drift nor vary when workloads or voltages change. The rotary shaker mechanism has a range of speeds between 50 rpm and 400 rpm. The reciprocating shaker mechanism has an adjustable stroke from 0 to 3½" and a speed range between 40 and 285 oscillations per minute.

OVERALL DIMENSIONS: Width 40", Depth 29", Height 65"
CHAMBER DIMENSIONS: Width 32½", Depth 21", Height 26"

UNCONDITIONAL ONE-YEAR WARRANTY

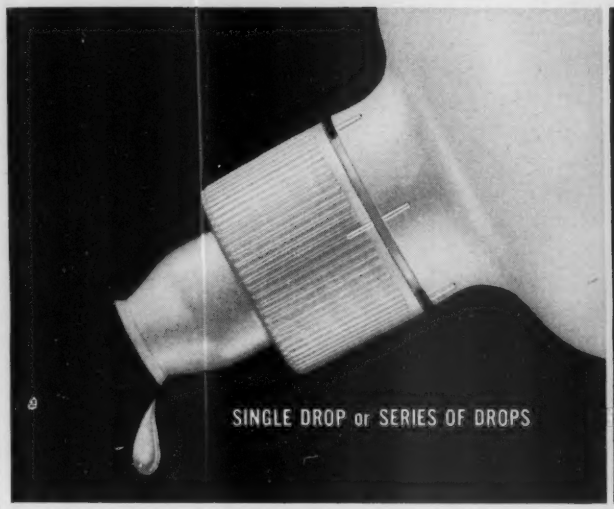
Write for Catalog G26S/5191

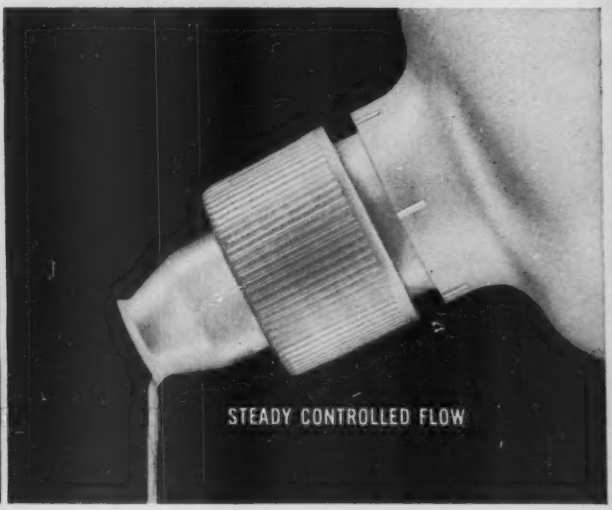
NEW BRUNSWICK SCIENTIFIC CO., INC.

PRECISION LABORATORY APPARATUS

P.O. BOX 606, NEW BRUNSWICK, NEW JERSEY

New ADJUSTA-FLOW HF bottle* lets you "dial" precise rate of flow





...another J.T. Baker first

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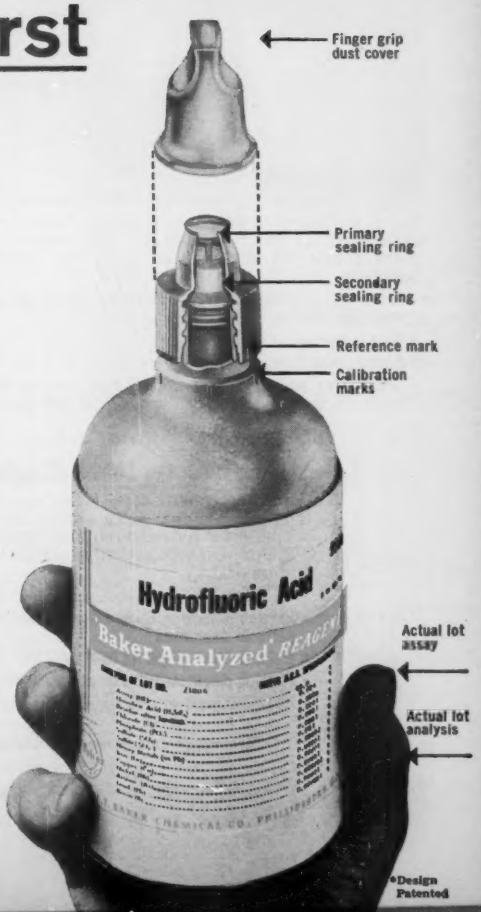

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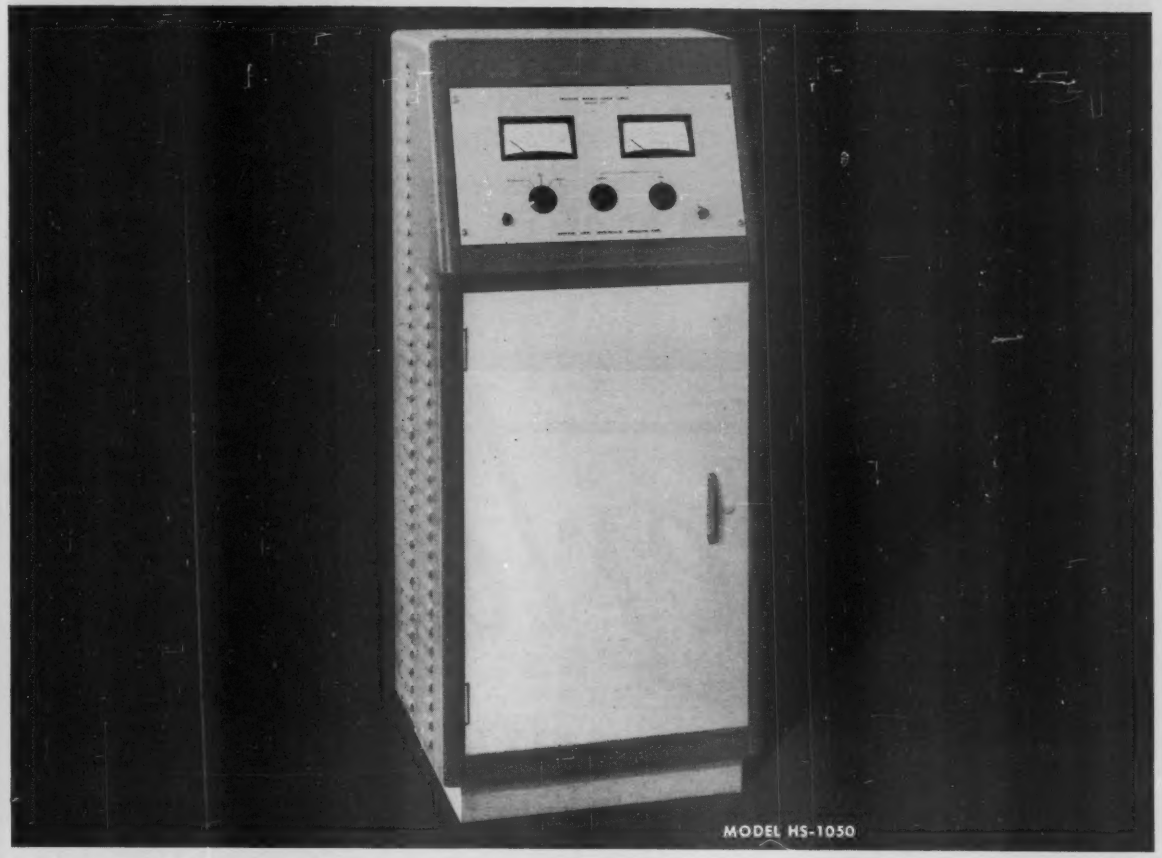

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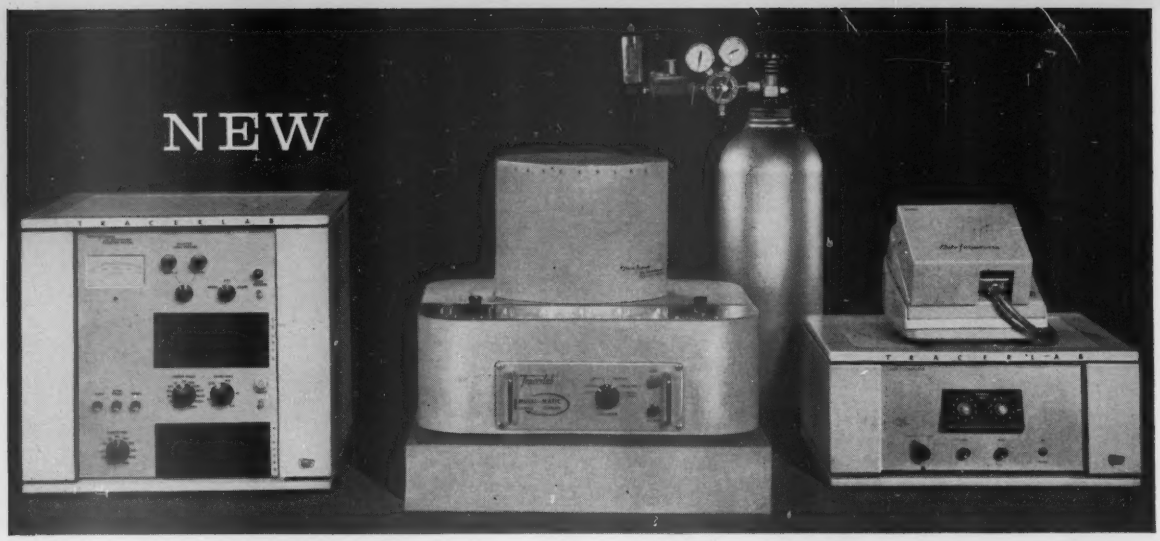

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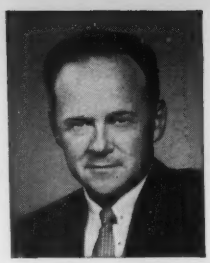

Studies in the Magnetic Properties of Thin Metallic Films

Temporary or transient memories of electronic computers consist of small doughnut-shaped ferrite cores hand-assembled into many complex matrices. Bulk, speed of response and costly manufacture create inherent limitations. It now appears possible to overcome these by replacing ferrite cores with tiny spots of magnetic film vapor deposited on a smooth flat surface.

Today's electronic computer has a memory which is part of the brain of the machine. Larger machines commonly have two memories: one for permanent storage of information, the other for temporary storage of more transient information. The temporary memory consists of a collection of ferrite cores, each core shaped like a tiny doughnut and having a number of wires laced between it and other cores forming a matrix or grid. The wires carry either the pulse of electricity which magnetizes the core, or a similar pulse which is the core's response to interrogation.

A series of these pulses, handled in a binary number system, have become the language of the computer. To function binumerically, circuits represent "0" by not conducting current, and represent "1" by conducting. Each memory core can be magnetized in one direction for "0", the opposite direction for "1." To avoid ambiguities, cores are made so they are not readily magnetized in any direction other than these two.

Each small ferrite core can be magnetized or interrogated in about a microsecond (one-millionth second). Unfortunately, the assembly of ferrite cores discourages automation processes, making manufacture slow and costly. In addition, the tremendous bulk of many millions of cores properly assembled prohibits machines requiring considerably larger transient memories.

Current basic research indicates that one of the most promising successors to the ferrite core is a tiny spot of magnetic film about 1,000 Angstroms (four millionths of an inch) thick, deposited on a smooth flat surface. These films have been prepared in Honeywell's Research Laboratories from an alloy of nickel and iron by heating the alloy until it vaporizes in a vacuum. Each freed vapor particle travels until it strikes a cooler surface. There it condenses and stays, if the surface is suitable and immaculately clean.

It might be assumed that the task would be simple. However, as the vapor condenses and becomes solid, it seems to become peculiarly sensitive to the nature of the surface on which it is being deposited. Unless oriented by a magnetic field (created by large coils that encircle the vacuum chamber), the films could be magnetized in a number of directions instead of along the desired single line. When we obtain uniformly bi-stable spots, we are in effect duplicating the action of ferrite cores. We also may use the same cycle by which bits of information are stored and extracted by reversing direction of the magnetic field.

The coercive force necessary to reverse (or "flip") the direction of magnetization within a thin film is very low. Another important advantage stems from the fact that reversal may be accomplished either by employing a rotational mechanism (simultaneous rotation of all atomic magnetic moments) or a wall-motion mechanism (sequential rotation of the atomic magnetic moments in the form of a moving wall). Both may be induced through application of a coercive force as small as one Oersted. Of the two mechanisms, rotational is much the faster; it makes possible the reading and writing of 100,000,000 bits of information per second on a single spot, as compared to about 100,000 for ferrite cores.

Honeywell scientists have consistently produced 256 bit (16x16) matrices uniform to plus or minus 5% of energy. Only this uniformity makes it possible to use the films in circuits, since a given small electrical pulse applied to any film must flip that film.

Uniformity has been achieved in part through study of deposition techniques and experiments both with various types of substrata and with various methods of cleaning them before deposition. It has resulted also, through broader understanding of the mechanisms involved, in causing reversal of the magnetic field. Even more important, however, have been detailed investigations into the factors that lead to non-uniformity, and subsequent development of techniques that tend to eliminate them.

The most difficult task remaining seems to be linking the film spots with printed circuits which will probably replace the wires used with the ferrite cores.

Our research on thin films is both basic and applied. Applied, since our scientists are trying to create better, faster, smaller memory systems for the commercial and military computers our engineers design; and basic, since they are trying to understand and explain all the phenomena described, as well as others that are completely baffling.

If you are engaged in magnetics research and would like to know more about Honeywell's work on thin magnetic films, you're invited to correspond with Dr. Richard Prosen, Honeywell Research Center, Hopkins, Minnesota. Or, if you would like a simplified explanation of the binary number system and how to perform standard mathematical manipulations using this system, write to Honeywell Research, Minneapolis 8, Minnesota.

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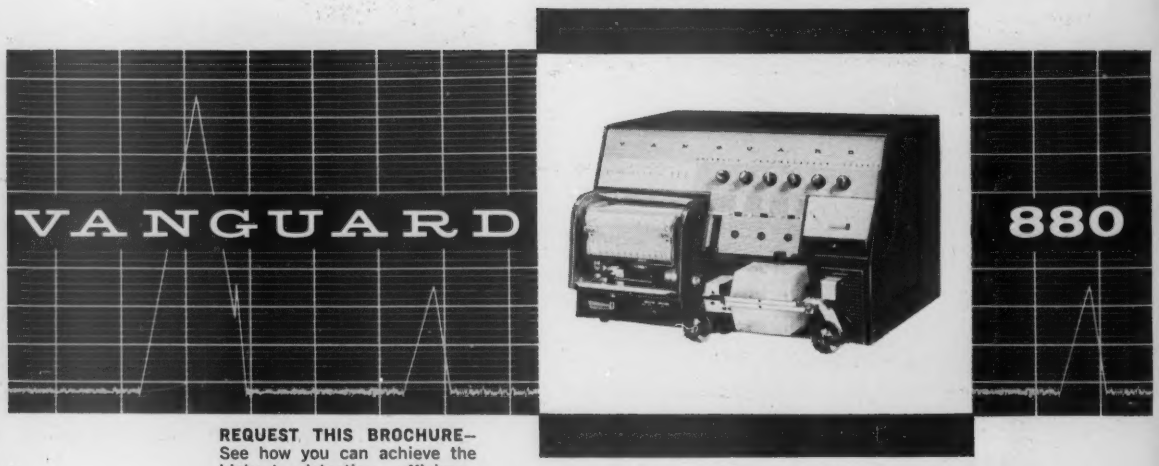
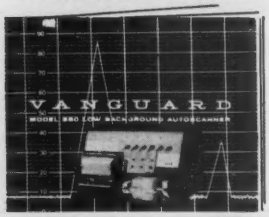
NEW STANDARD ACCESSORY, TOO! Also available is the new, exclusive Vanguard Model 880ADS, a completely automatic system for quantitative integration and digital presentation of radioactive zones. For complete details concerning either the Model 880 or Model 880ADS, please write or call.



REQUEST THIS BROCHURE—See how you can achieve the highest detection efficiency available for chromatogram scanning. Booklet outlines distinctive features and lists all operational characteristics of the Model 880 AUTOSCANNER.



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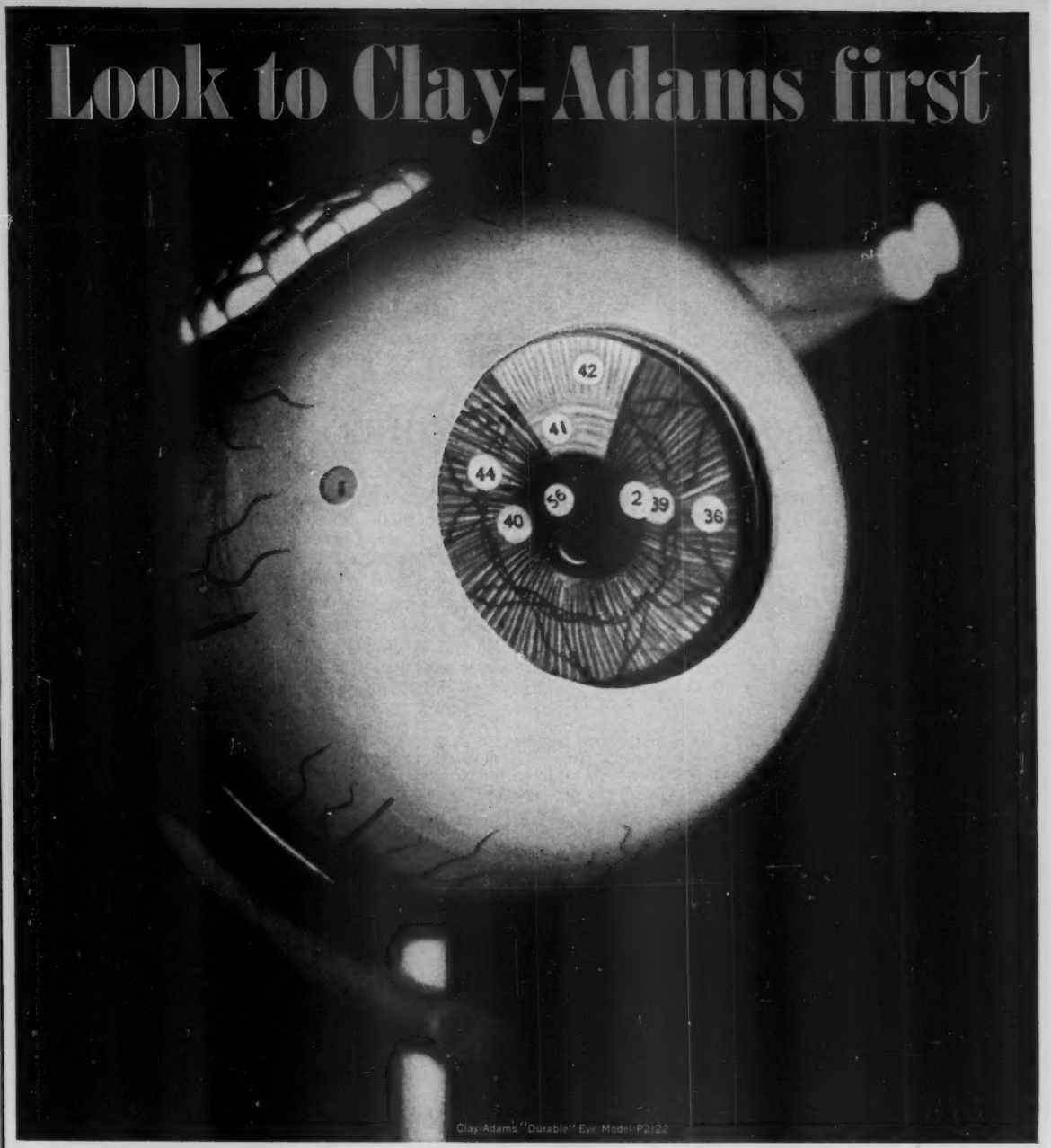

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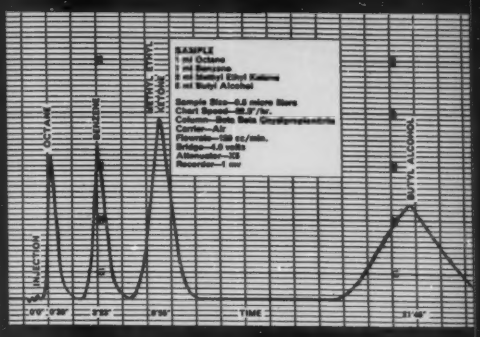

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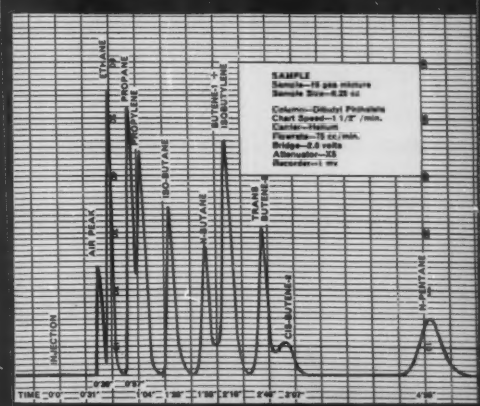

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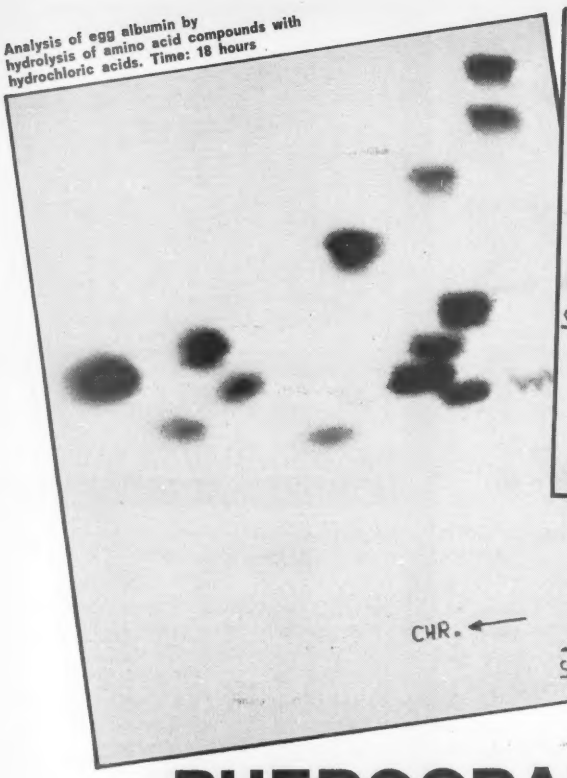

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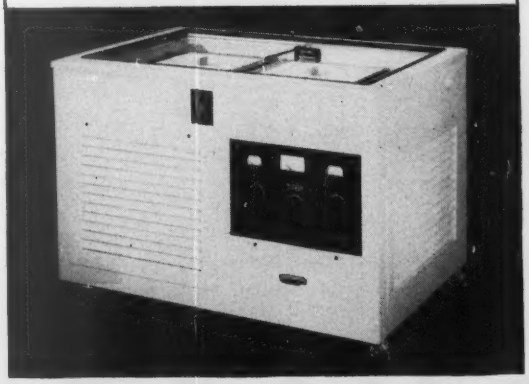

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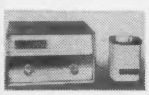

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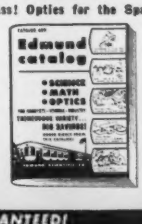

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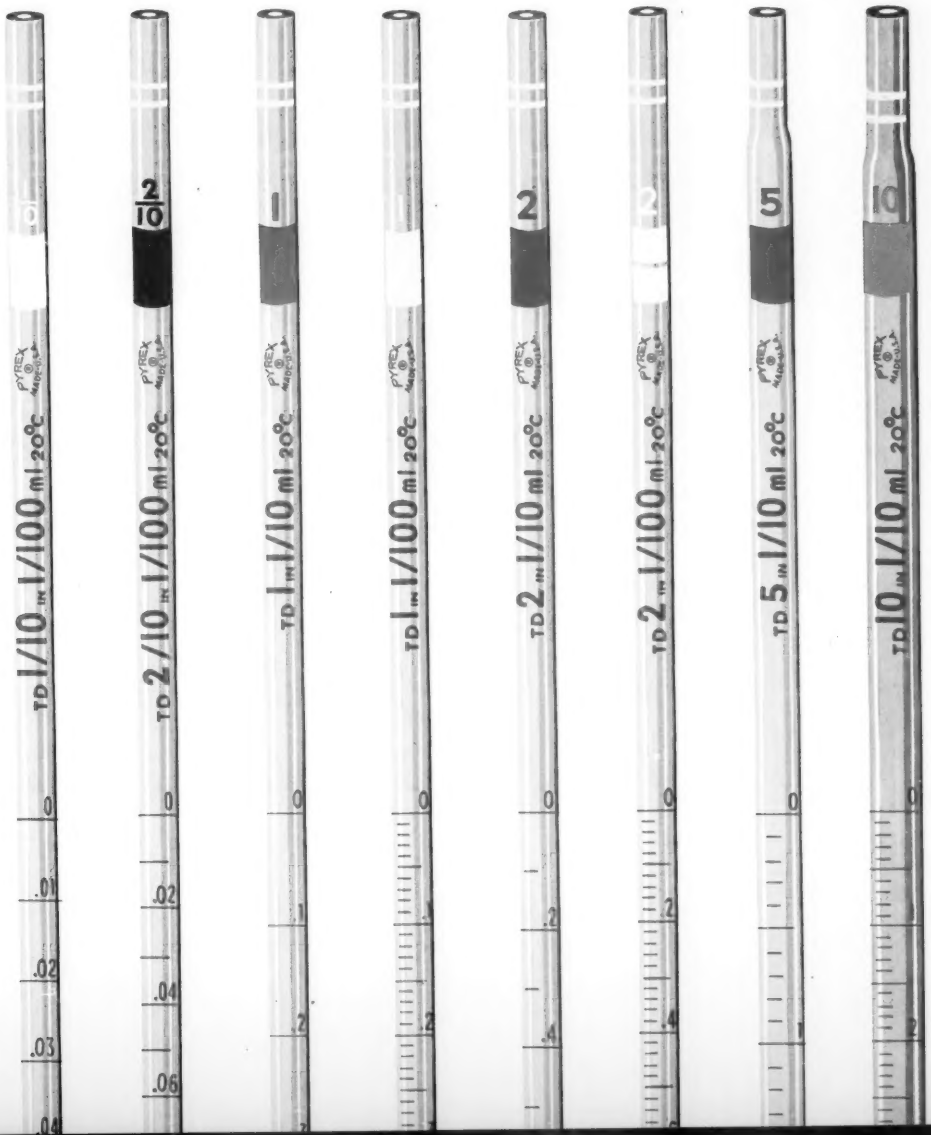
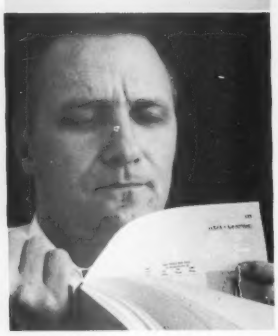


No. 1010
Beaker with handle



Hmm, a beaker with a handle. Makes sense. Should lift and pour as easy as a coffeepot – even when it's boiling hot. Won't need those clumsy tongs now.*

No. 5960
Flowmeter

Four orifices on this flowmeter. From ¼ to 2 mm. Says you just turn the stopper to select the one you want. That's pretty simple.*

No. 3690
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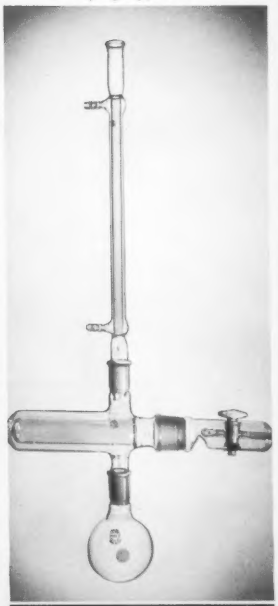
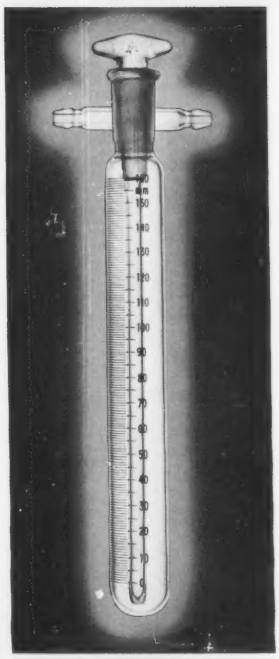
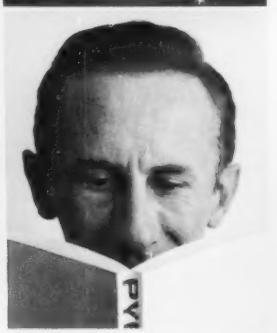


Drying apparatus for small quantities. Works at constant temperature, under reduced pressure. I won't have to buy separate parts and make my own after all.*

No. 6952
Manometer





A "t" manometer. Scaled from 0 to 160 mm. The scale is red for easy reading. Available in single outlet also. Real easy to fill.*

*MORAL?

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For high-temperature work up to 1500 degrees C., there's a VYCOR brand glass beaker of 96% silica.

Whatever you need, check the quantity discount allowances. They run as much as 23.5%. And, if you don't have a copy of our new catalog, LG-2, write for yours.

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Additional card on page 1627

READERS' SERVICE 19 May 1961

Print name and address on front of card

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1517	1535	1553	1611,O	1621	1633,U	1642,O	1651,I	1659,O	Cover 4
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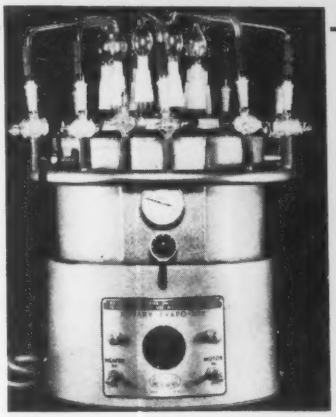

FEATURES

- Internal variable voltage transformer controls vibration rate.
- Simultaneously evaporates 10 test tubes or centrifuge tubes, 16-25 mm. diameter.
- Connects to efficient water aspirator as vacuum source.
- Automatic thermoregulator controls temperature of water bath.
- Controlled circular vibration creates deep swirling to prevent "bumping" during heating under vacuum.

With the Rotary Evapo-Mix it is possible to achieve in a single test-tube evaporation rates for water of 1.0 ml/min; ethanol, 4.2 ml/min; methanol, 4.5 ml/min; ethyl acetate, 7.2 ml/min; and for acetone 10.0 ml/min., with all 10 test tubes attached, evaporation decreases approximately 25%.

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Request Bulletin S3-2100 for complete details.

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The development, field test, and customer acceptance of a versatile radioisotope manual liquid scintillation counting assembly has been completed. Here is a system for Carbon 14 and Tritium that incorporates simplicity and flexibility – that is in the price range of all laboratories.

Among its field proven features: simplified circuitry which assures reliable operation, negligible field maintenance; the high voltage supply, scaler and timer are all standard B/A instruments – they can be used with other systems and tailored to your special needs; specially designed, dark preadaption sample holder accepts a variety of sample configurations and sizes; the system is compact and versatile.

These – and other features of the Model 745 Liquid Scintillation unit – are available at the lowest cost you would expect to pay for reliable performance with liquid scintillation. You owe it to your budget to get more information – write or call your nearest B/A representative today.

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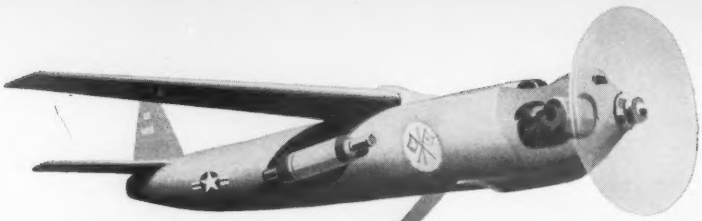
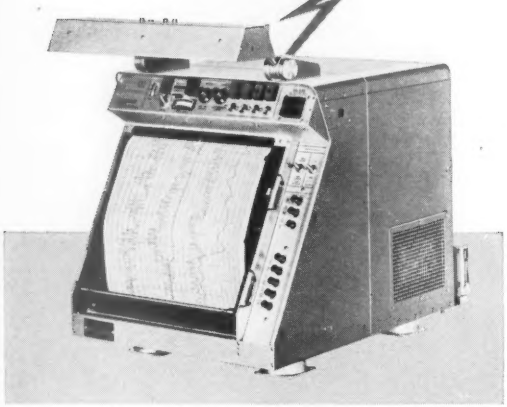
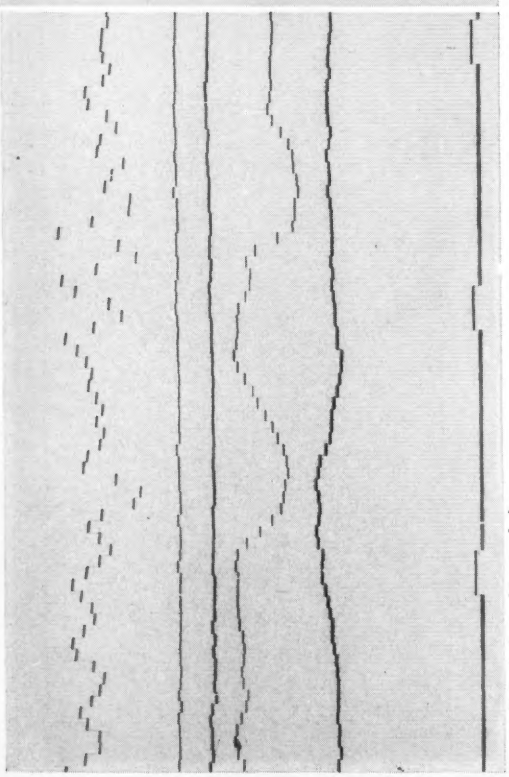


Tracking a Surveillance Drone with the Visicorder





Record shown ¼ actual size.

Drone surveillance and reconnaissance gives U.S. Army combat units a high-altitude vantage point with much broader horizons from which to view battlefield action and terrain.

If effective use of the data gathered by the drone—the "eye in the sky"—is to be made, accurate instruments have to be on hand to monitor the drone's position and movement, its operational behavior and its response to flight commands. Telemetry supplies the radio link which transmits all this behavior information to a thoroughly-instrumented mobile tactical command post developed by Tele-Dynamics Division of American Bosch Arma Corp.

The Honeywell Model 1012 Visicorder has been selected as the direct readout unit in the Tele-Dynamics Drone Surveillance Telemetry system. In use with its companion instrumentation, the 36-channel Visicorder simultaneously displays the 22 channels of information required to track a drone, plus the timing traces.

In the Tele-Dynamics van, which serves as a tactical command post, the Visicorder provides both an instant "quick look" and a permanent record of the drone's operational parameters.

Signals are transmitted over a single channel by time-multiplexing. Signal and battery strength, engine speed and temperature, pitch and roll commands, altitude, airspeed, attitude (pitch and roll), yaw, acceleration (horizontal and vertical), and angle of attack are recorded by the Visicorder, along with three separate records of vibration.

Like the other units of the Tele-Dynamics system, these Honeywell Visicorders are built for rugged service . . . to deliver the data . . . when the drone is up and the chips are down.

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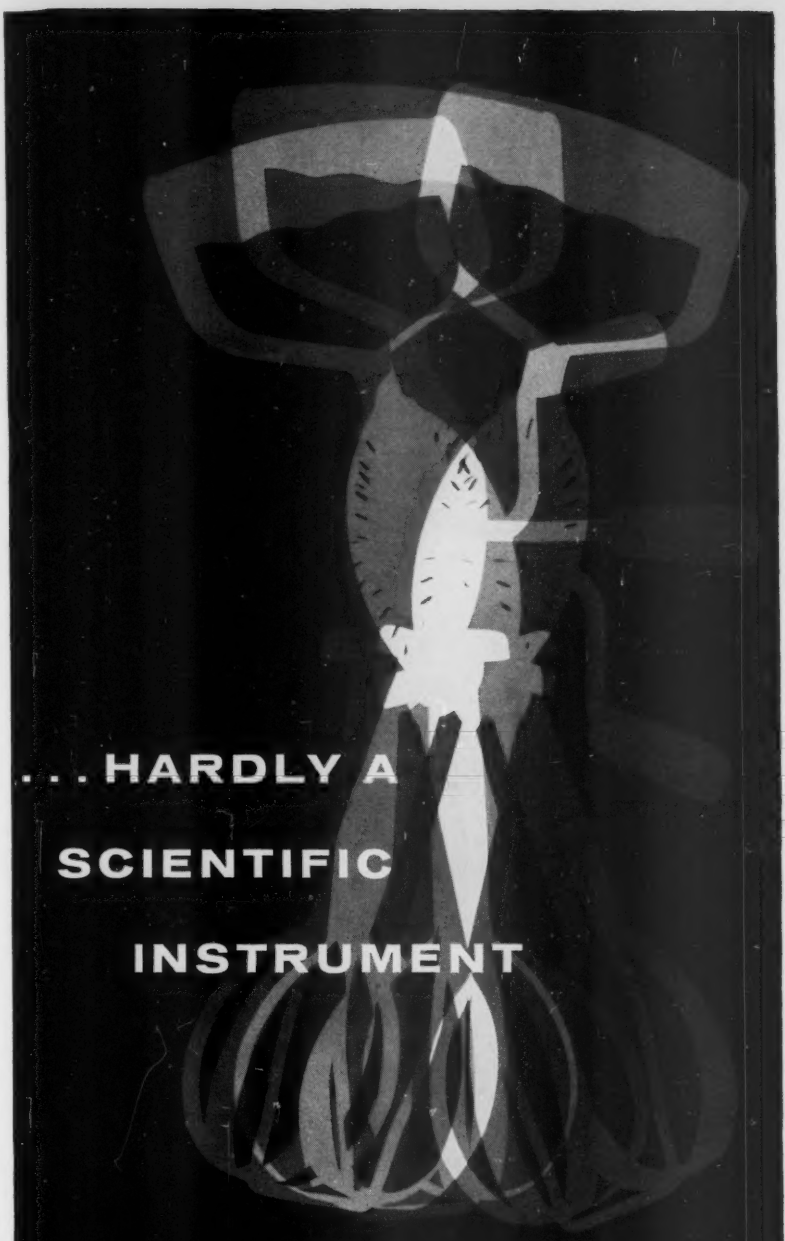

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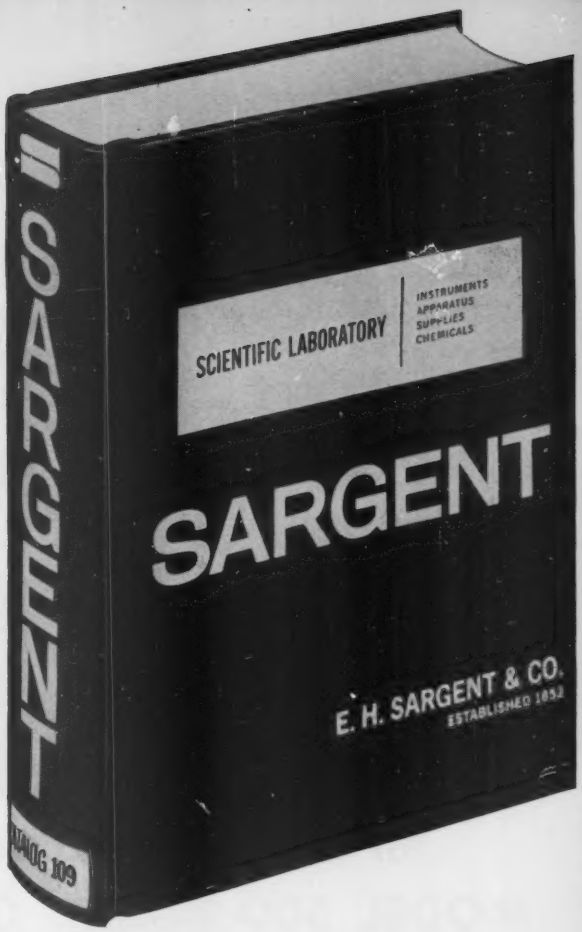

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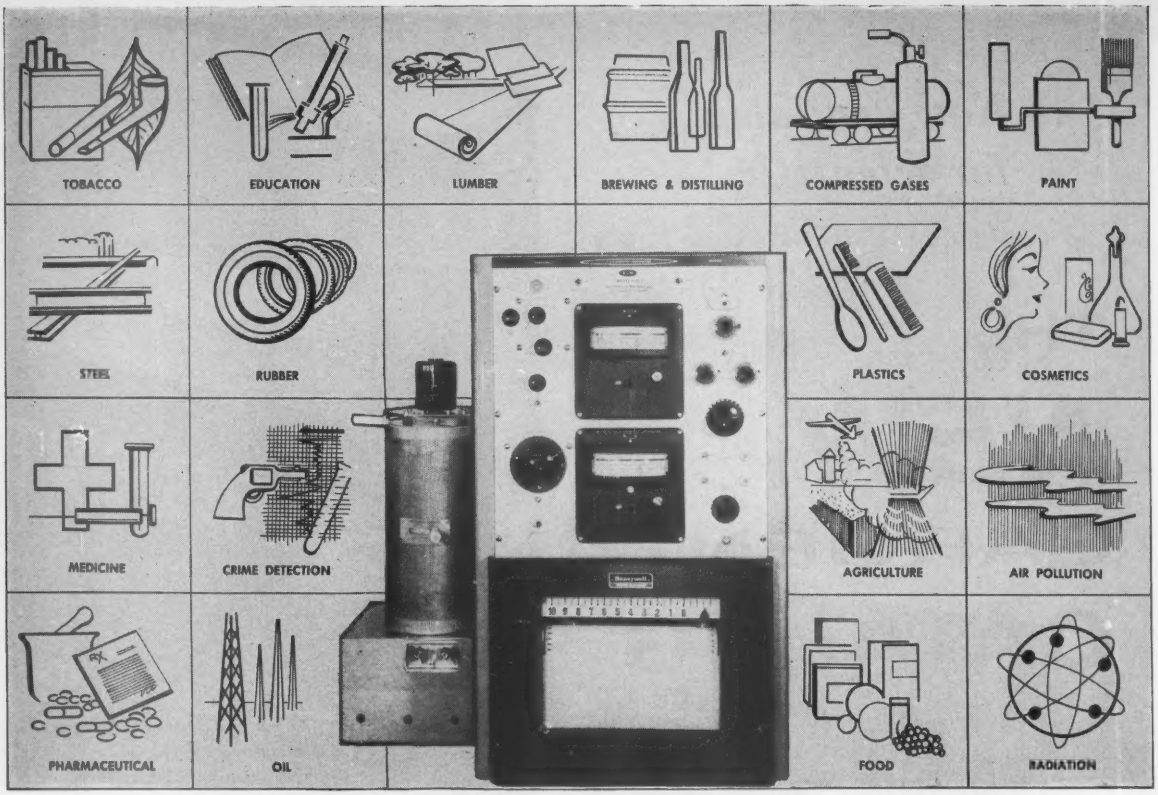


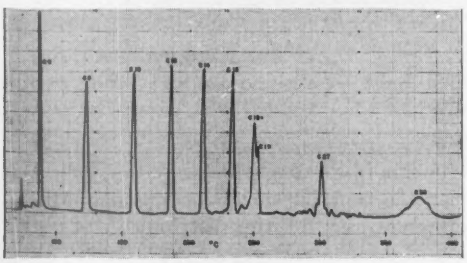
For Research and Development in all Industries

MODEL 500 LINEAR PROGRAMMED HIGH TEMPERATURE GAS CHROMATOGRAPH

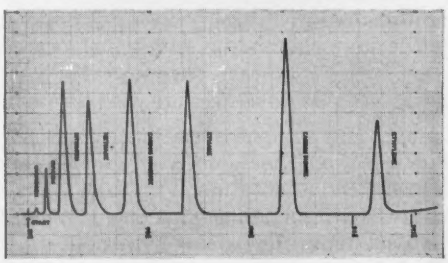
The technique of linear programmed temperature gas chromatography combined with high temperature operation makes possible these previously difficult analyses: (1) wide boiling mixtures; (2) gases (H_2, O_2, N_2, CO, CO_2 and light hydrocarbons); (3) trace quantities of high boiling residues in low boiling solvents; (4) correlation of retention times with analytical distillation data; and (5) temperature sensitive materials.

Two such analyses are shown below. Note that with programming, both separations required only one column. Without programming, the methyl ester separation would have required a fixed high column temperature with a loss in resolution of the lower boiling components. In the separation of gases at a constant column temperature, ethane, carbon dioxide and ethylene would not have eluted. To chromatograph this mixture at constant temperature, two columns and two samples are necessary.

With a Model 500, column scouting is easily performed because columns can be changed and baselines re-established in ten minutes. Heating from room temperature to 300°C takes about 10-15 minutes (27 minutes to 500°C) and cooling back to room temperature takes about the same time. Model 500 has separate and independent temperature controls, each to 500°C, for column, detector, and injection port. In addition, eighteen linear heating rates from 0.29 to 42°C/min. provide a wide selection for optimum column conditions.



CONDITIONS:
COLUMN TYPE: 2 FT. SILICONE GUM RUBBER
COLUMN TEMP. PROGRAMMED AT 5.6°C/MIN. FROM 75°C to 400°C.
This is a typical programmed temperature chromatogram demonstrating a separation of methyl esters with widely differing boiling points. Observe the sharp peaks, shaped nearly alike.



CONDITIONS:
COLUMN TYPE: 4 FT. MOLECULAR SIEVE 5A
COLUMN TEMP. PROGRAMMED AT 4.0°C/MIN. FROM 35°C TO 247°C.
By programming column temperature, H_2, N_2, O_2, CO, CO_2, ethane, and ethylene all separate on a single column in one run. Using the same column, the programmed temperature technique works equally well for the separation of nitrous and nitric oxide in the presence of these gases.

Similar analytical advantages with Model 500 apply to the other industries illustrated above. Write for our bulletins on Model 500. These describe in detail all the specialized analyses.

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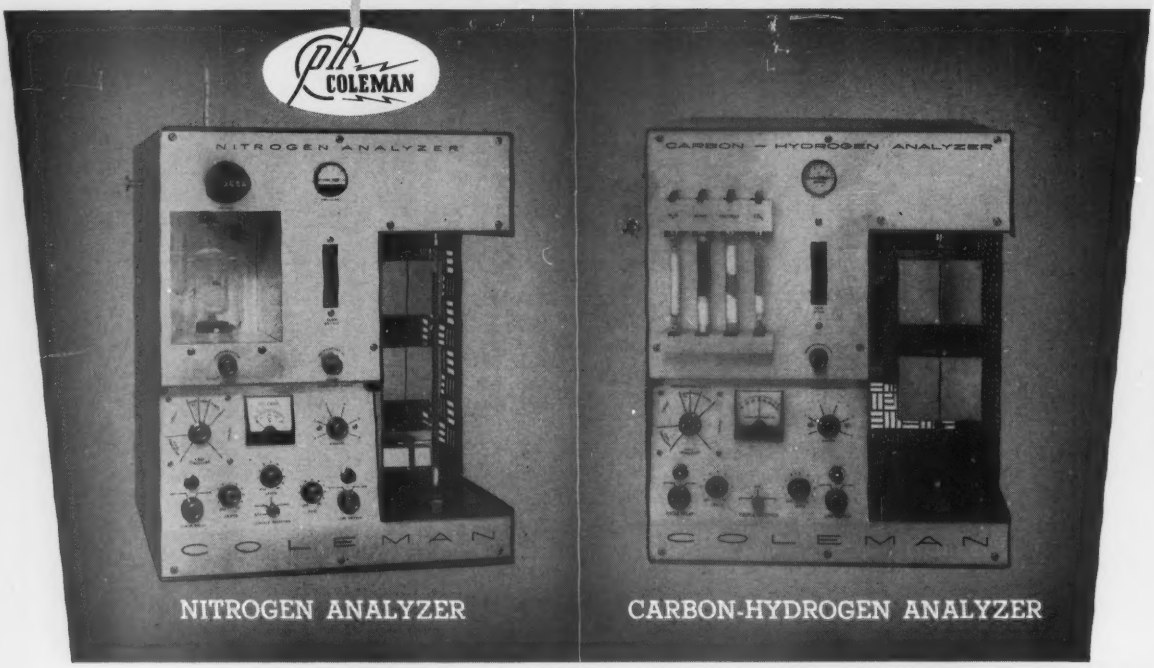

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The Nitrogen Analyzer burns the sample and selectively absorbs the combustion products, except Nitrogen. Nitrogen is collected in a micrometer-driven syringe providing direct volume readout in microliters.

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Both instruments provide vast improvements in reproducibility over traditional methods . . . variations in operator technique are eliminated as a source of analytical error.

Other important benefits common to both instruments:

SPEED—instrument operating cycle is only 8 minutes.

ACCURACY—results of routine nitrogen and carbon-hydrogen determinations agree with theoretical percentages ±0.2%.

ECONOMY—each instrument takes only 18 inches on laboratory bench.

VERSATILITY—Sample sizes range from 1 to 100 mg; sample substances include virtually all homogeneous materials that burn below 1100° C.

Put automated combustion analysis to work in your laboratory . . . ask your instrument dealer for a demonstration.

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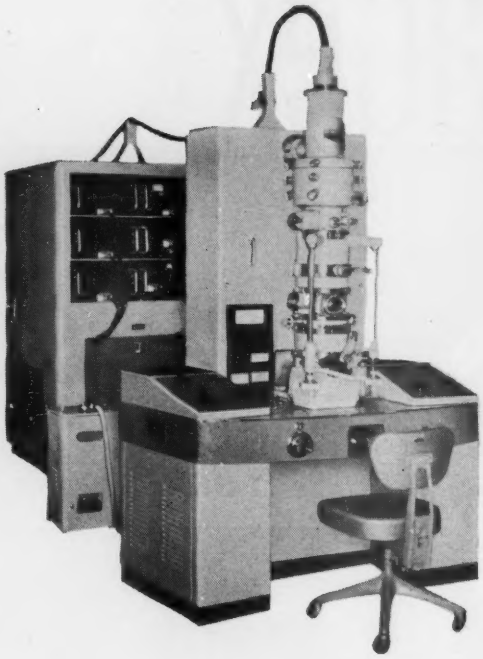

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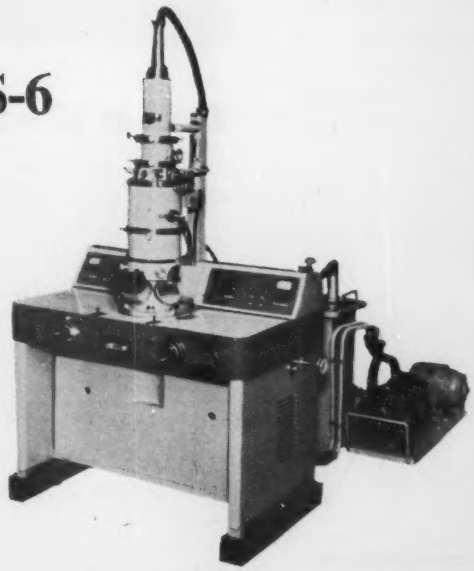

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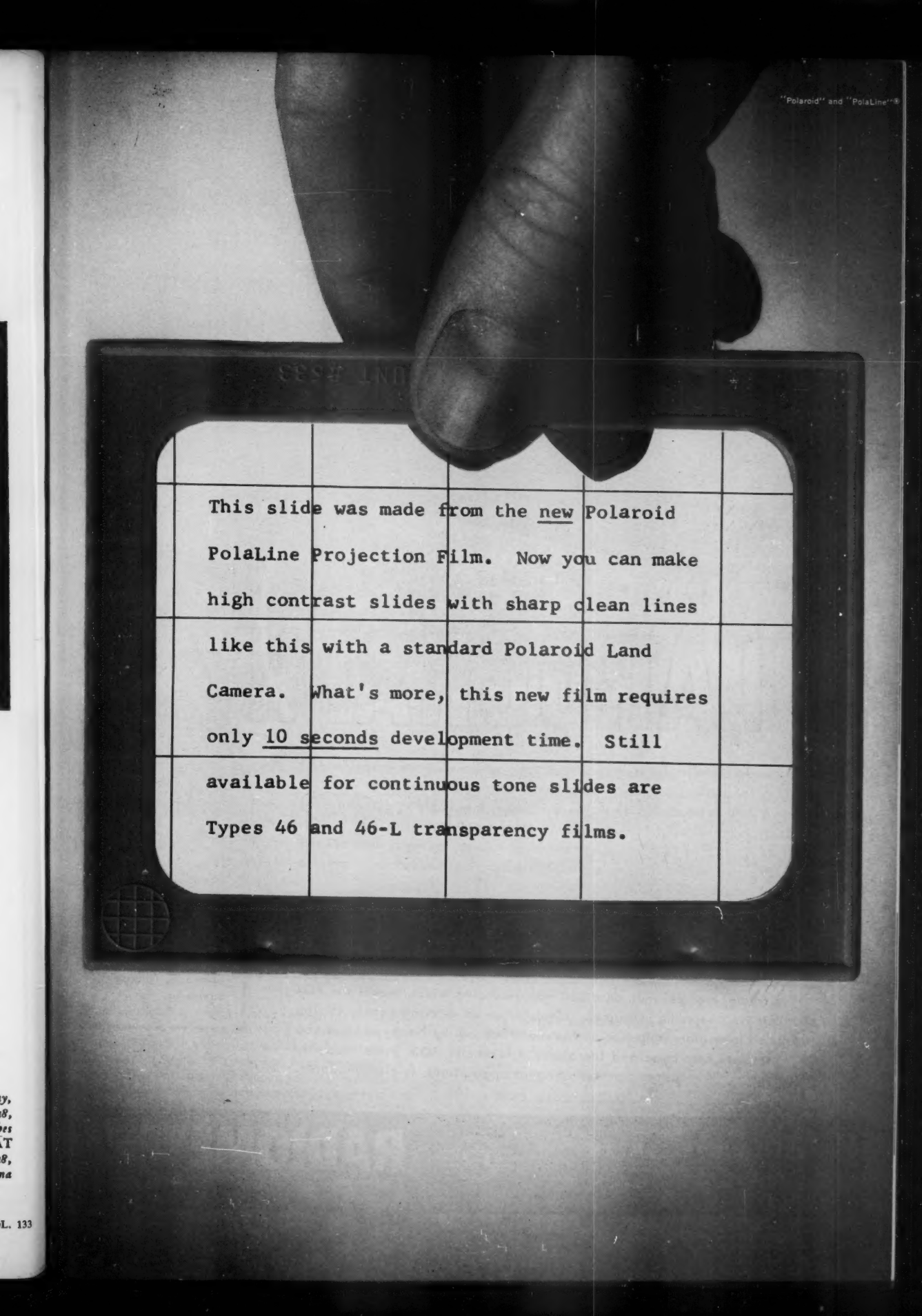
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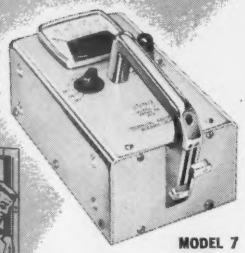

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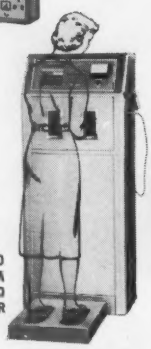

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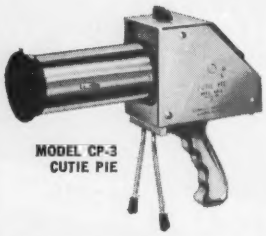

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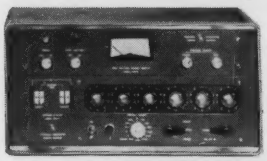

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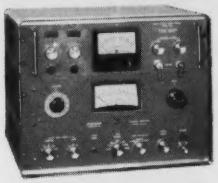

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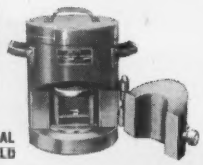

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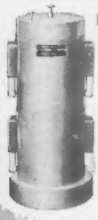

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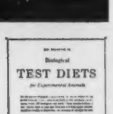

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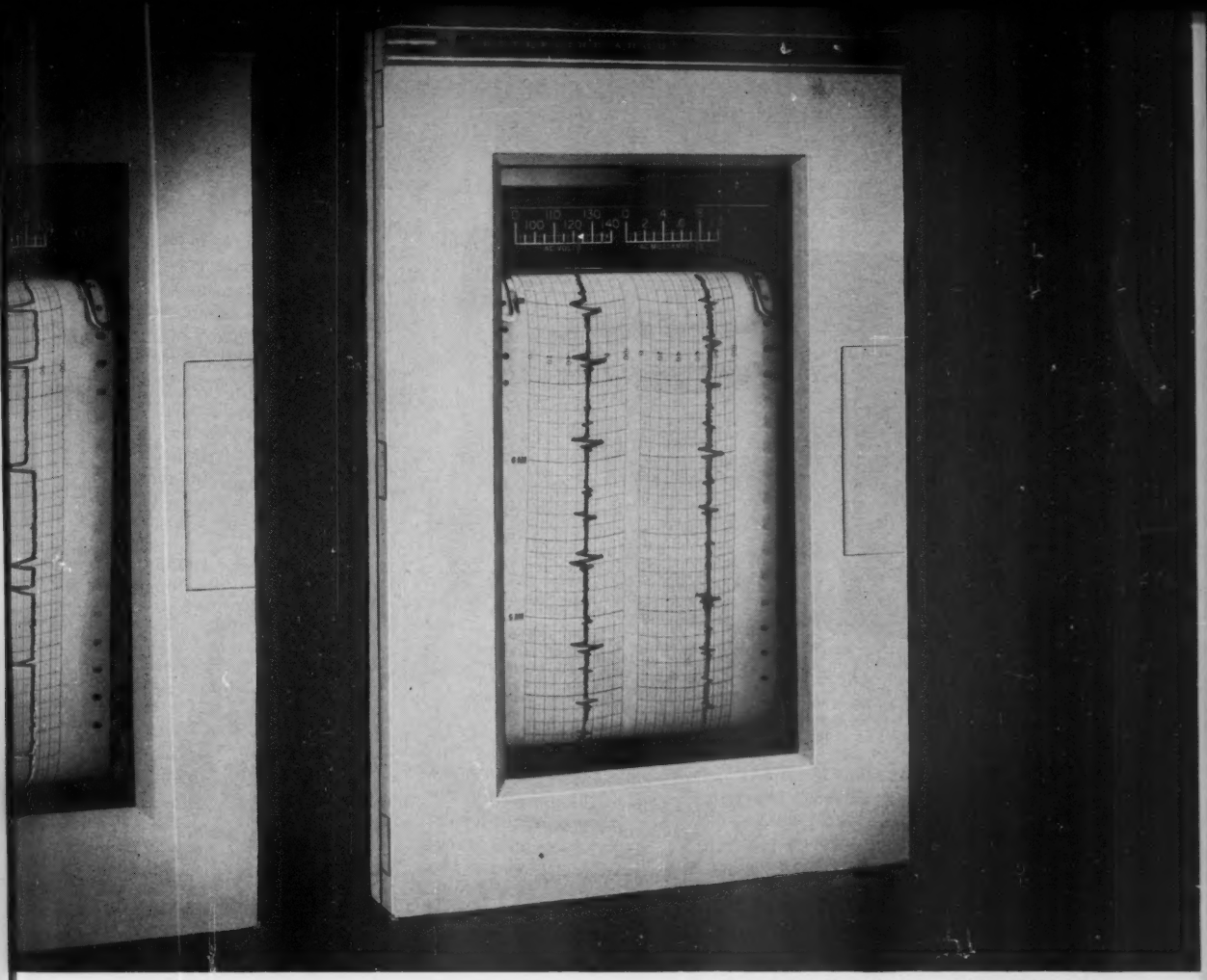

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19 May 1961, Volume 133, Number 3464

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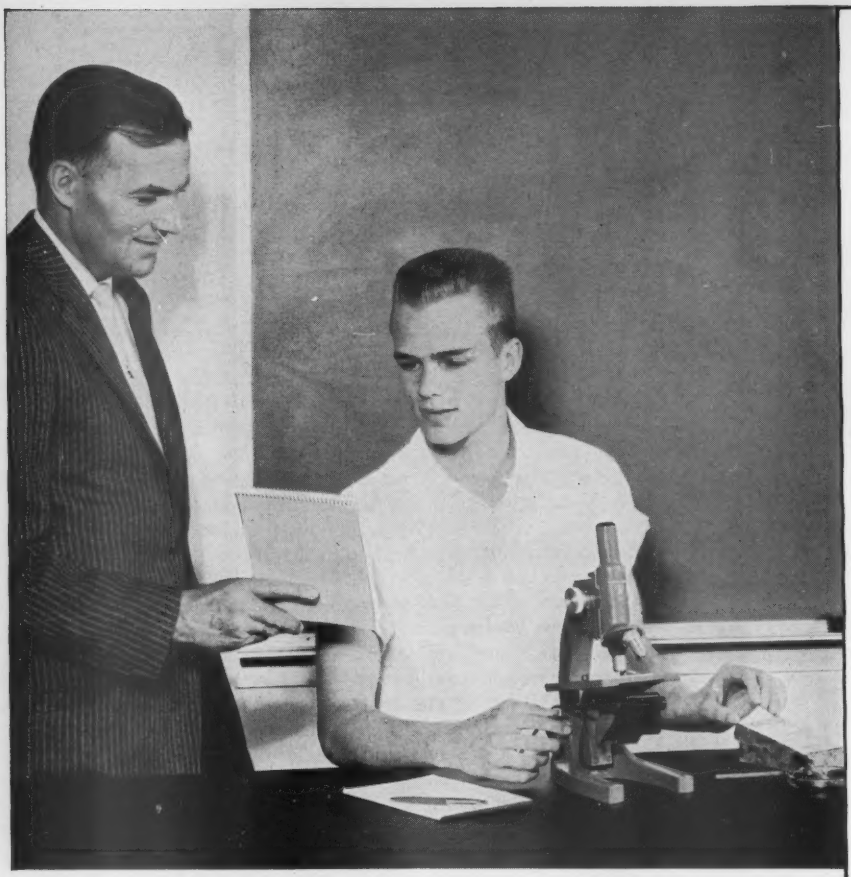
Diploma Diplomacy

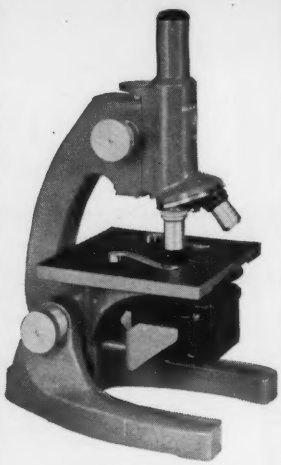
In his recent *The Voice of the Dolphins*, a slim volume of commentary on contemporary affairs in the form of science fiction, Leo Szilard, of the Enrico Fermi Institute of Nuclear Studies, at one point predicts the outcome of one part of our program of educational assistance for less-developed countries. The prophecy is that our fellowship program for bringing African students to this country, educating them in American colleges, and then sending them home, a program that began last year with action by the Kennedy Foundation and the State Department, will grow through the years to produce an Africa that is developed but that is unyielding in the ill will it bears us. The basis for this prediction is the expectation that the visiting students, who will be the leaders of the new Africa, will be treated by white Americans with the same courtesy they give Negro Americans.

As a piece of political satire, this example of an unexpected and undesired consequence of educational assistance may not be entirely successful. The fact is that if we look to the present, instead of the future, the difficulty that is already upon us is rather the reverse. In the matter at least of advanced scientific training of many of our foreign students, the trouble is not what attitudes our visitors display toward us on their return but their reluctance, in view of the educational and research facilities they enjoy here, to return at all. But if Szilard's example is not altogether convincing, it makes a valid point. In educational assistance and in other parts of public affairs, we frequently get into trouble because we are not prepared, as we are in scientific investigation, to explore the consequences of our ideas.

A proposal has been offered, however, that meets both the contingency that the scholars we have helped will not like us and the contingency that they will like us only too well. Arthur F. Burns, professor of economics at Columbia University, has suggested that instead of bringing students to this country we send the universities to them, instead of importing students we export universities. The suggestion was offered in a brief speech given last year at the University of Chicago and now published under the title "Why not diploma diplomacy?" in the first number of that university's new magazine, *Context*. Burns suggests that we build universities, for those countries that ask for them, in which such professions will be taught as engineering, agriculture, medicine, and public administration. He also proposes that we provide as much assistance in the way of staffing these institutions as is wanted.

This suggestion was offered in the context of the more general problem of financial grants and easy loans to other countries, and Burns finds that it compares favorably with other forms of economic aid. The cost of the universities could be met by using some of our present funds for economic aid. From the viewpoint of foreign policy, the building of universities on foreign soil, Burns says, would be free of the distrust of our motives that characterizes some of our other efforts at aid, as when, for example, because of the deficit in our international accounts, we seek arrangements requiring that the credits we offer be spent on American goods. From the viewpoint of economics, and this from a professional economist, the benefits of building universities could repay the cost a thousand fold.—J.T.





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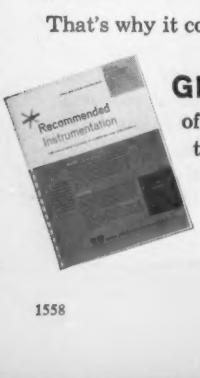

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19 May 1961, Volume 133, Number 3464 SCIENCE

INSTRUMENTS AND TECHNIQUES

Animal Cell Cultures

Tissue culture is a powerful tool in the study of nutrition, physiology, virology, and genetics.

Norman P. Salzman

The usefulness of animal cell cultures as an experimental tool in such diverse fields as biochemistry, biophysics, genetics, virology, and cancer research has become increasingly evident in recent years. Nevertheless, many workers have been deterred from using this tool by the belief that the technique presents formidable manipulative difficulties and requires a heavy commitment of time for routine maintenance of cultures. Perhaps these were once important considerations. However, with the simplified procedures now available, animal cell cultures represent a technically simple biological system of major importance.

In this article I describe the simple and reliable cell-culture techniques presently being used in this laboratory which have made possible biochemical, genetic, and virus studies. Many of the specialized procedures used for cell propagation or for the initiation of primary cell cultures are not described, since this information is contained in several books and articles which have recently been published (*1*). Some of the findings are discussed, and a few of the present uses of cell cultures are cited.

In general, the manipulations involved in cell culture are similar to those employed in a bacteriological laboratory. The various fluids must be sterilized, as must all the glassware, and aseptic technique must be used. The addition of penicillin and streptomycin to the medium greatly facilitates the maintenance of sterile cultures. Maintaining duplicate stock cultures, which are fed with separate batches of media, is a further protection against loss of stocks due to contamination.

Perhaps the best assurance against the loss of strains is a repository of frozen stock cultures. Cells frozen in a serum containing growth medium supplemented with 5 to 20 percent glycerol and stored at −70°C yield viable cultures, when thawed, even four years later (*2*, *3*). Stocks grown from frozen cultures may further serve as controls for cultures which have been serially propagated for long periods and in which a new cell type may have emerged as a result of genetic selection.

Culture Media

A major development in recent years has been the replacement of growth media containing ascitic fluid, serum, plasma, and tissue extracts by simple, chemically defined media supplemented with low concentrations of dialyzed serum. This has permitted meaningful and reproducible biochemical experiments. The composition of the basal medium developed by Eagle and his associates, currently in use in this laboratory, is shown in Table 1 (*4*). This medium will support the growth of monolayer and suspension cultures of both normal and malignant cells, most cell cultures growing with an 18- to 26-hour generation time. It has also been used in the establishment of primary cultures from human biopsy material (*5*). Each of the components present in it has been demonstrated to be essential for growth, and the omission of any single component will result in cell death. The six amino acids glycine, alanine, serine, glutamic acid, aspartic acid, and proline, which are not components of the medium, can be shown to be formed *de novo*, the first three deriving their carbon atoms from glucose and the latter three, from glutamine (*4*). Similarly, the purine and pyrimidine components of ribonucleic acid and deoxyribonucleic acid can be formed *de novo* (*4*, *6*). With most serially propagated strains, the addition to the medium of the six nonessential amino acids or purines or pyrimidines or their derivatives does not significantly stimulate growth. However, their presence may alter the enzymatic composition and potential of these cells, their presence acting to suppress those enzymes involved in their *de novo* formation (*7*).

Among the essential amino acids only glutamine is interconverted and thus utilized for the biosynthesis of other cellular components. Its carbon skeleton serves as the primary source for the synthesis of the pyrimidines (*8*) and of glutamic acid, aspartic acid, asparagine, and proline (*4*). Further, its amide nitrogen is utilized directly as a source of purine and pyrimidine nitrogen (*8*), and its amino nitrogen serves as the source of the amino nitrogen of the nonessential amino acids (*4*). The failure to detect significant levels of incorporation into the nonessential amino acids when various uniformly C^{14}-labeled essential amino acids are present in the medium suggests that the latter are not degraded to any signifi-

The author is a member of the staff of the Laboratory of Cell Biology, National Institute of Allergy and Infectious Diseases, National Institutes of Health, Bethesda, Md.

cant extent. It is not clear whether those enzymes involved in the catabolism of the essential amino acids are present but fail to express themselves, whether these enzymes are diluted out in a rapidly growing culture, or whether, in the establishment of primary cultures, cells containing these enzymes are at a selective disadvantage and are rapidly eliminated from the culture.

The classification of amino acids as essential relates only to cell growth and survival and is not informative at the enzymatic level. At least one of the "essential" amino acids, cysteine, can be synthesized *de novo* from methionine and glucose but is still required for cell survival, suggesting that its rate of biosynthesis is insufficient to permit either cellular proliferation or maintenance of cell viability (*9*). For other amino acids—namely, threonine, valine, leucine, isoleucine, glutamine, arginine, histidine, lysine, methionine, phenylalanine, and tyrosine—the present indication is that the requirement reflects the inability of the cell to synthesize these components *de novo*. However, citrulline (but not ornithine) has been shown to substitute for arginine (*4, 10*), and the keto analogs will in most cases substitute for the corresponding essential amino acids, indicating the cell's ability to carry out a limited number of steps which are required in the synthesis of the essential amino acid (*4*). [The ability of keto acids to substitute for the corresponding amino acids, however, may only reflect the lack of specificity of the transaminases which are present in the cell (*11*) but whose primary role is in the synthesis of the nonessential amino acids.]

Whether the failure to synthesize these essential amino acids is the result of single or multiple metabolic blocks is at present not known. While identical amino acids are required for the growth of a variety of normal and malignant cells (*12*), it is nevertheless possible that the specific metabolic blocks in the synthesis of a particular amino acid differ from cell to cell; and under these circumstances one might find syntrophism between different cell lines in a medium lacking a single essential amino acid. Even more remarkable than the qualitative similarity in amino acid requirement among various cell lines would be the finding that cultures deriving from normal and malignant tissues of both human and animal species possess identical patterns of biochemical lesions in the synthesis of each of the essential amino acids. It is possible that cultures with discrete biochemical lesions are presently available, and that the presence of such lesions will be revealed by detailed biochemical analysis.

Table 1. Minimum essential medium for cultivation of mammalian cells in either monolayer or suspension cultures. Optional supplementation is of two kinds: (i) nonessential amino acids (glycine, alanine, serine, proline, glutamic acid, aspartic acid, asparagine), each at 0.1 m*M*; (ii) sodium pyruvate (1 m*M*).

Compound	Concentration (m*M*)	Amount (mg/lit.)
	L-Amino acids	
Arginine	0.6	105
Cystine	0.1	24
Glutamine	2.0	292
Histidine	0.2	31
Isoleucine	0.4	52
Leucine	0.4	52
Lysine	0.4	58
Methionine	0.1	15
Phenylalanine	0.2	32
Threonine	0.4	48
Tryptophan	0.05	10
Tyrosine	0.2	36
Valine	0.4	46
	Carbohydrate	
Glucose	5.5	1000
	Salts	
NaCl	116	6800
KCl	5.4	400
$CaCl_2$	1.8 (0)*	200 (0)*
$MgCl_2 \cdot 6H_2O$	1.0	200
$NaH_2PO_4 \cdot H_2O$	1.1 (11)*	150 (1500)*
$NaHCO_3$	23.8	2000
	Vitamins	
Choline		1
Folic acid		1
Inositol		2
Nicotinamide		1
Pantothenate		1
Pyridoxal		1
Riboflavin		0.1
Thiamine		1
	Others	
Penicillin		50
Streptomycin		50
Phenol red		5
	Serum protein	
Supplied as 5- to 10-percent whole or dialyzed serum		

* For suspension cultures.

Serum Requirement of Animal Cell Cultures

Human, equine, and bovine sera have been most commonly used as a medium supplement. Prior to use, the serum may be dialyzed at 5°C for 24 hours. The sodium chloride concentration is then readjusted to 0.85 percent, and the serum is sterilized by filtration through a Selas No. 03 filter and added to the medium at a final concentration of 5 to 10 percent. Since human serum from a particular donor may prove somewhat toxic, sera from ten individual donors are routinely pooled. Such pooled serum has not been found to be toxic. A similar procedure can be followed with sera from other species; alternatively, the serum from a single donor can be pretested to determine its suitability—for example, to support growth, to support virus replication, and so on. The choice of serum species is dictated in part by availability and in part by the type of study being carried out. The possible presence of viral antibodies in human serum, for example, precludes its use for certain virus studies. No significant differences in gross chemical composition or in metabolic behavior have been observed when HeLa cells grown in dialyzed human serum and in dialyzed horse serum were compared. However, striking morphological changes have been observed to be serum-dependent, and reversible (*13*).

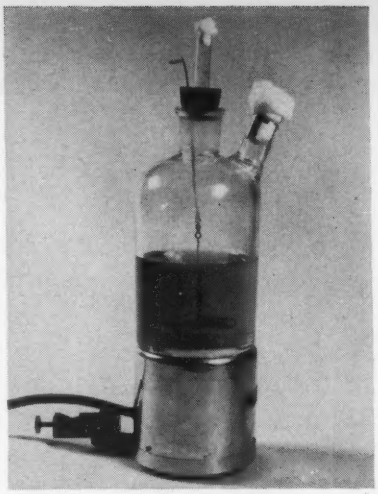

Fig. 1. Assembly for growth of animal cells in suspension.

Dialyzed serum can be stored at 4°C for several months and still remain active in supporting growth. However, proteolytic enzymes which are present in serum will cause hydrolysis on storage even at this temperature, resulting in a significant level of contamination of the serum by peptides and free amino acids (*14*). Breakdown is not detected in serum which is dialyzed, sterilized by filtration, and then stored in the frozen state prior to use.

A great deal of effort has been directed towards understanding the role of serum protein in animal cell nutrition. It has been reported that only two purified fractions are essential for the growth of the HeLa S3 cell, serum

albumin and a glycoprotein (*15*). While a chemically defined medium supplemented with these two protein fractions will support clonal growth of HeLa S3 cells, it does not suffice for the clonal growth of human euploid cell cultures (*16*). Work in other laboratories indicates that neither protein is an absolute requirement for cell attachment or for growth of many cell lines. Variants have been obtained from human and mouse cultures and have been serially propagated in a chemically defined medium by procedures which involve frequent detachment from glass and subsequent reattachment (*17*). In other cultures, where no selection for a particular cell type has occurred, other proteins or factors derived from protein can be substituted for serum. Thus, products released on the digestion of serum by the crude proteolytic enzyme preparation Viokase diffuse through a semipermeable membrane at a rate which permits sustained growth of suspension cultures of all cell lines tested (*18*). Similarly, monolayer cultures have been successfully grown in a medium supplemented with lactalbumin hydrolyzate and salmine (*19*).

The present evidence for mass populations would indicate no absolute requirement for any specific protein. Since serum-free media which support growth have contained varying types of degraded protein—that is, bacto-peptone (Difco), lactalbumin hydrolyzate, or the dialyzable products of Viokase-treated serum—it seems unlikely that they are supplying to the cells an identical protein degradation factor of any significant size. It seems more likely that the contribution of these various peptide products, or of serum, is that of providing a source of material of low molecular weight which is bound to them, and which they gradually release to the cell.

However, recent reports by Rappaport *et al.* (*20*) suggest quite a different role for protein or factors derived from protein. The HeLa, L, and primary monkey-kidney cells were successfully grown in monolayer cultures in a chemically defined medium without a prolonged lag period required for cell adaptation or selection. Successful growth was dependent on proper pretreatment of the glass surface with alkali. The data indicate that such treatment modifies the net charge on the glass surface, and further, by establishing a reservoir of Na^+ ions, permits continued cell attachment even with the diffusion of protons from the cells which occurs as a result of cell growth and metabolism. These results suggest that protein does not have a nutritional role but, rather, is important in producing a physically compatible cell surface, which is required in both monolayer and suspension cultures and which, in the former, permits attachment of cells to the glass.

Establishment of Cell Cultures

Well over 100 cultures have been successfully initiated from tissues, both normal and malignant, of a number of animal species and maintained in a rapidly growing state for prolonged periods. The procedure for initiating cultures involves mincing of the tissue, treatment with a proteolytic enzyme (usually trypsin) in order to disperse large fragments and liberate single cells, and inoculation of the tissue digests into a suitable growth medium. Procedures have been reported which permit the establishment of cultures from extremely small amounts of tissue with consistent success (*21*). Moreover, subcultures have remained euploid over a period of one year, during which time they were maintained in an actively growing state. Unfortunately, with few exceptions, the specialized functions characteristic of the organ of origin are not present in the serially propagated culture. For reasons which are not yet clear, cultures initiated with cells derived from liver do not convert phenylalanine to tyrosine, nor does the mouse fibroblast culture produce collagen.

I indicated previously that many cultures had been compared and that identical nutritional requirements were demonstrated, regardless of the origin of the culture. However, when cultures were compared for their ability to produce tumors in the cheek pouch of the hamster, cultures derived from malignant tissues could be distinguished from cultures derived from normal tissues; the latter produced tumors only when 100 times as many cells were injected as were required for tumor production with cells from cultures derived from malignant tissue (*22*). In this study there was no indication that cultures derived from normal tissues regularly become malignant after prolonged maintenance under conditions of rapid growth. These studies further argue against the idea that all cultured cells dedifferentiate into a common, primitive cell type.

The similarity in nutritional requirements of serially propagated cultures may depend on the fact that the techniques which are used in establishing

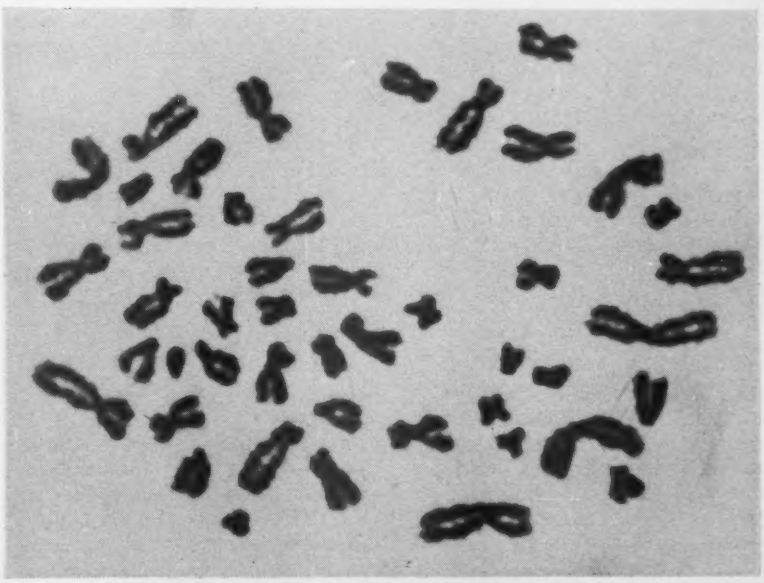

Fig. 2. The chromosomes of human lung fibroblasts grown in vitro. The cultures were primary explants, carried out by Rune Grubb (Institute of Bacteriology, Lund, Sweden), of tissue taken from human embryos. The chromosomes were studied a few days after the in vitro explanation of the biopsy material, from an acetic orcein squash preparation pretreated with colchicine and hypotonic saline. The photomicrograph [from Tjio and Levan (*35*)] shows an early metaphase. These cultures were the first in which the number of chromosomes in man was found to be 46 and not 48.

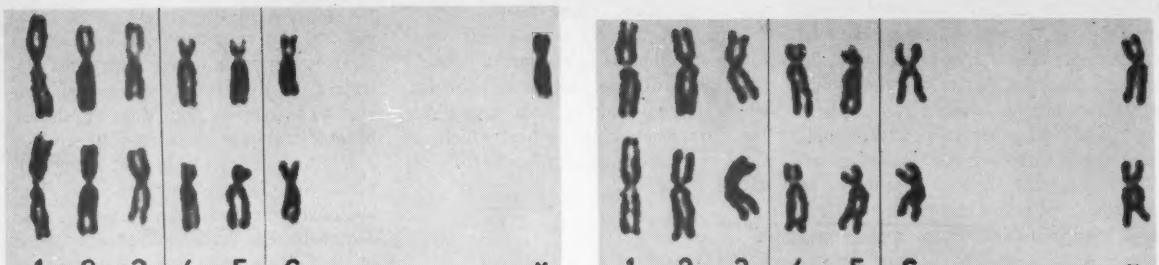

Fig. 3. The human male (left) and female (right) karyotypes. Photomicrographs of the individual chromosomes were cut from enlarged photomicrographic prints (original enlargement, × 4000), paired, and arranged according to a standard system of nomenclature [*Am. J. Human Genet.* **12**, 384 (1960)] (about × 2840). [Courtesy of J. H. Tjio]

them all select for the most rapidly proliferating cell types, cells dividing at a slower rate soon being lost from the population. Methods which would eliminate the rapidly proliferating cells might conceivably allow for the emergence of new types of cell cultures. A possible approach is provided by the observation that 5-bromodeoxyuridine is incorporated into the deoxyribonucleic acid of animal cells, this incorporation finally proving lethal (*23*). The presence of 5-bromodeoxyuridine in the medium for varying periods when cultures are established would result in its preferential uptake by the rapidly growing cell. These cells would then fail to grow when the inhibitor was subsequently removed or when its action was reversed by the addition of thymidine, and thus selection for the more slowly dividing cells could occur.

Techniques for Cell Maintenance

For routine maintenance, cells are grown in stoppered flat-bottom flasks. The culture is divided when the cells form a confluent sheet. Used medium is decanted, and the cells are removed from glass by mechanical scraping with a rubber-tipped policeman into fresh medium, or by treatment with a Versene or trypsin solution (*1*). The first procedure is the simpler for routine maintenance. Clumps are dispersed by pipetting the suspension back and forth, and a suitable aliquot is added to a bottle containing fresh medium. Routinely, 1/10 to 1/20 of the cells is used as an inoculum. These cultures are fed on the third and the fifth day and are again divided on the seventh day. With more frequent feeding a somewhat more rapid rate of cell proliferation can be obtained.

When a series of replicate flasks is required for an experiment, it is desirable to remove cells from glass with either trypsin or Versene. This gives well-dispersed suspensions, which are then centrifuged and resuspended in fresh medium. They are continuously agitated with a magnetic stirrer, and equal aliquots are dispensed with a Cornwall pipet into flasks containing the desired medium. Cells from suspension cultures may also be used to prepare monolayer cultures, and in this case preliminary dispersion of the cells is not required.

Suspension cultures (*24*) can readily be initiated from monolayer cultures with many cell lines, though some cell lines have been found to be unsuitable, due to excessive clumping. Cells from a monolayer culture are dispersed into a medium modified to permit growth of suspension cultures (for its composition, see Table 1) and transferred to a spinner flask (Fig. 1). The rate of stirring is adjusted to maintain a slight vortex at the surface of the liquid. The cultures are initiated at a population of 100,000 cells per milliliter, the volume of fluid being sufficient to keep the bottle almost half full. Lesser volumes permit too rapid a loss of carbon dioxide, while too large a volume results in excess acidity due to trapping of carbon dioxide. The cultures are diluted every second day, and cultures maintained between 1×10^5 and 5×10^5 cells per milliliter can be kept indefinitely in the log phase of growth. External control of the *p*H has not been found necessary for cells grown in this manner.

The behavior and characteristics of monolayer and of suspension cultures are not equivalent, and the particular study may dictate which is to be preferred. Steady-state growth conditions cannot be obtained in monolayer cultures but may be obtained in suspen-

sion cultures, which will remain indefinitely in the logarithmic phase of growth if the cultures are suitably diluted (see above). In monolayer cultures the size of the inoculum will determine the length of time cells can be maintained in the log phase, while the glass area of the bottle in which the cells are grown will determine the maximum cell population that can be achieved.

These monolayer cultures display three distinct phases of growth—a lag, a logarithmic, and a stationary phase. Further, monolayer cultures show systematic fluctuations in their cellular content of deoxyribonucleic acid, ribonucleic acid, and protein, levels of the latter two decreasing sharply with increasing age of the culture (*25, 26*). As would be expected, the relative rates of synthesis of ribonucleic acid and deoxyribonucleic acid, as measured by uptake of C^{14}-labeled nucleic acid precursors, is also dependent on the age of the cultures (*25*). Good reproducibility from experiment to experiment is thus dependent on the age and metabolic state of the culture and may be difficult to achieve. Similar wide variations in response to virus infection have been shown to be dependent on the metabolic state of the culture (*27*). However, the ease with which media can be decanted without disturbing monolayers and subsequently replaced by media containing any desired test substance is a distinct advantage. The techniques available for readily preparing large numbers of replicate monolayer cultures and measuring the changes in cell mass make monolayer cultures ideal where the effects on cell growth of a large number of agents at several concentrations are being evaluated (*28*).

Suspension cultures do provide a source of cells in a highly reproducible metabolic state. Through proper dilution schedules, these cells can be maintained in the logarithmic phase of growth and show a constant content of ribonucleic acid, deoxyribonucleic acid, and protein per cell (*29*). Since aliquots can be removed from a single culture, this obviates the need for planting replicate cultures. Further, cells are collected readily by centrifugation, rather than by the more tedious and traumatic procedures involved in harvesting cells from glass. Finally, cells can be harvested and resuspended at any desired population, so that effects of varying cell population may be evaluated.

Cloning of Mammalian Cells

The technique for the production of clones from single cells, as originally described, required x-irradiated nondividing cells which served as a feeder layer and which permitted the formation of clones from single viable cells (*30*). Subsequent studies, however, demonstrated that a feeder layer was not necessary and that growth could be initiated from single cells in a suitably constituted medium (*13*). The medium described in Table 1, when supplemented with serine alone, or with all the nonessential amino acids, has been demonstrated to be satisfactory for this purpose (*31*).

To produce clones, suspended cells are counted in a hemocytometer, diluted to a suitable concentration, and then plated in petri dishes. The dishes are incubated in a humidified incubator at 37°C in air containing 5 percent carbon dioxide, and visible clones form within seven to ten days. At this time they may be stained and counted, or the cells in a single clone can be harvested and used to initiate a new mass culture.

Genetic Studies with Human Cell Cultures

The availability of serially propagated cultures and mutant cells derived from them would suggest the possibility that a genetic analysis of human cells could be made, in the hope that transformation and genetic recombination could be demonstrated and that individual genes could be assigned to linkage groups and the order of genes on a single chromosome established. However, those cultures which have been maintained in an actively growing state for several years have all been shown to be aneuploid (*32*). While these aneuploid cultures exhibit a principal karyotype, they do contain a significant percentage of cells with other karyotypes. Further, mutants selected from a parent culture for drug resistance, morphological differences, or the ability to grow in a more limited serum-containing medium gave rise to cultures with karyotypes distinct from the parent culture—a finding which suggests that phenotypic variability of aneuploid cell populations may result from changes in

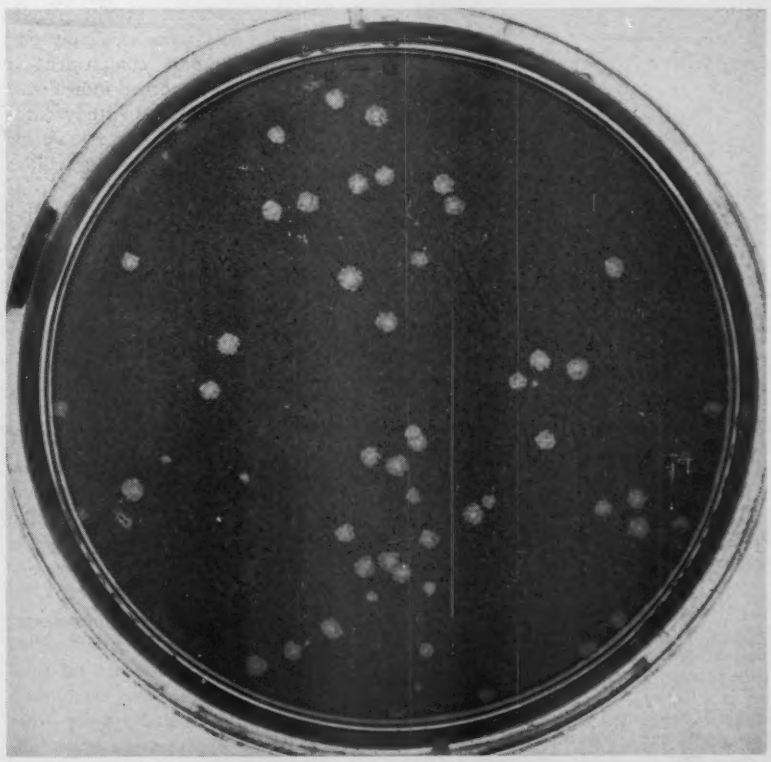

Fig. 4. Plaques produced by vaccinia virus on chick embryo monolayers. Tissue from ten-day-old embryos was used to prepare the monolayers, which were used 48 hours later. The dish shown above is stained with neutral red; the plaques are four days old.

the chromosomal constitution of the cells (*33*). In view of these drawbacks of aneuploid cultures, and since it has recently been shown that euploid cultures can be established and maintained under conditions of rapid growth for prolonged periods (*21*), these cultures would seem better suited for such experiments.

Euploid cultures initiated with tissue from skin biopsies of galactosemic individuals have been found to exhibit the characteristic metabolic lesion (*34*). The inability of these cells to grow in a galactose-containing medium, in contrast with the normal or heterozygous cultures, should make detection of transformed galactosemic cells possible, even if such transformation occurs with low efficiency.

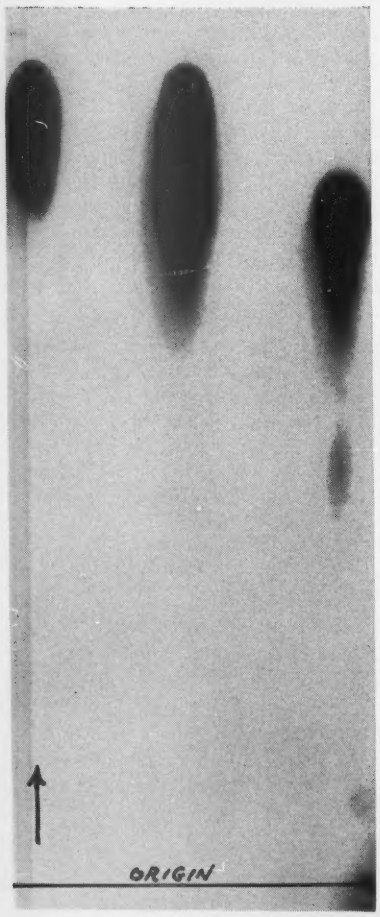

Fig. 5. A chromatogram of antitumor substances. The location of the active materials is detected through the use of KB cells in agar. The left band contains pluramycin (100 μg); the middle band, pluramycin (100 μg) and quinocycline (100 μg); the right band, quinicycline (100 μg). Descending chromatography was carried out in an ethanol, acetic acid, water (5:1:19) solvent system. [Courtesy of George B. Whitfield, Upjohn Company]

With the finding, by Tjio and Levan (*35*), that in human lung fibroblast cultures established from aborted embryos the true chromosome number of somatic tissue was 46 rather than 48 (Fig. 2), there was renewed interest in cytological observations on human cells. Since the report of Tjio and Levan, many additional observations on the human chromosome complement have been made. These studies have employed both short-term bone-marrow cell cultures and serially propagable fibroblast cultures established from solid tissue biopsies. While observations obtained by the two procedures have been similar, the procedure involving serially propagated cells has the potential advantage of permitting biochemical studies which may be related to the chromosome karyotype. To date there has been almost universal confirmation of the finding that the normal chromosome number in man is 46 and, further, that homologous autosomes from cells of different individuals are not significantly different in length or in the position of the centromere (Fig. 3). This close agreement in the results from several laboratories, indicating that the chromosome complement is identical in different normal individuals, has encouraged workers to study chromosomes in cases where there was prior indication that genetic abnormalities might exist.

Persons with chromatin-negative Turner's syndrome have been found to lack one sex chromosome, having only 45 (*36, 37*). In such patients, who are phenotypically female, no testes are present, the "gonads" consist of slender streaks of connective tissue, most secondary sex characteristics fail to develop at puberty, and other characteristic abnormalities are present. Males with chromatin-positive Klinefelter's syndrome—a condition in which spermatogenesis is scanty or lacking, the testes are small, and certain female traits are present—have 47 chromosomes, with an XXY complement (*38*). Females with the XXX complement, analogous to the "superfemale" *Drosophila*, have also recently been reported (*39*). These studies have established that in man, unlike findings for *Drosophila*, the Y chromosome is an important determinant of maleness, and that the XO and XY complexes are not equivalent.

An examination of the chromosome complement in cultures from patients with mongolism has revealed an extra small acrocentric autosomal chromosome (*40*), thought to arise as a result of nondisjunction during oogenesis. It must be noted that a variety of inherited conditions which have been examined have revealed a normal chromosome complement [for example, phenylketonuria and Gaucher's disease (*37*)].

Animal Viruses

The animal virus–animal cell system is presently being explored with many of the techniques which had previously been applied to the bacteriophage–bacteria system. While there are many similarities, the differences between the bacterial and mammalian systems indicate that qualitatively different observations will result from a study of animal viruses. The occurrence of both ribonucleic acid–containing and deoxyribonucleic acid–containing animal viruses, the widely varied responses of cells to virus infection, and the possibility of determining the role of the subcellular structural components in the various stages of virus replication are some of the factors which make this an extremely attractive area of investigation. Viruses capable of eliciting tumor production in animals have been detected by means of cell cultures, and the mechanisms of the malignant transformation of cell cultures by viruses are under investigation (*41*).

In general, cell cultures have proved a powerful tool in the detection of a wide variety of previously unrecognized viruses. Light and electron microscopy and fluorescent antibody and radioautographic techniques make possible direct observations of intracellular viral development. Further, precise quantitation can be achieved by the use of the plaque assay system (*42*) (Fig. 4). The results of the many studies with animal viruses are described in recent reviews (*43*).

Cytotoxicity and Detection of Tumor-Inhibitory Substances

The effects of a number of compounds known to have antitumor activity in animals or man have been studied in cell cultures. A good correlation between cytotoxicity and anti-

tumor activity was demonstrated, and the results suggested the usefulness of cell cultures as a primary screen in the detection of antitumor agents (*44*). Procedures which depend on cytotoxic effects on animal cells have been developed for the isolation of such compounds from fermentation beers. The bioautographic and disk assay procedures, with animal cells embedded in or growing on agar, furnish methods for the rapid quantitation of the cytotoxic agents and for their identification (*45*). In the bioautographic technique, beers or other solutions are chromatographed, and the paper chromatogram is then placed in contact with agar previously seeded with cells. The diffusion of cytotoxic material from the paper and the resultant cell killing are detected with an appropriate viable cell strain. A typical bioautograph is shown in Fig. 5. The relative mobility of a cytotoxic agent in several different solvent systems aids in its characterization.

Similar techniques have proved of great value in the isolation of a large number of antibiotics from fermentation beers, and it is likely that growth-inhibitory materials highly active against animal cells will also be obtained. The examination of these compounds with respect to their antitumor activity in man will be of great interest.

The foregoing discussion illustrates the broad range of problems in which animal cell cultures are currently being used. While the papers from a single laboratory often tend to stress the importance of particular procedures in use in that laboratory, there is no evidence that the minor variations from laboratory to laboratory are of great importance, or that complex procedures are indeed more effective than the more direct one. It seems likely that new and improved cell-culture techniques will be forthcoming, in view of the larger number of investigators who are presently using cell cultures. For example, most studies with animal cell cultures have, until recently, utilized aneuploid cells. Procedures which have recently been described permit the establishment and maintenance of rapidly growing euploid cultures (*21*), and it seems likely that these cultures will now be more widely used. Euploid cultures have yielded important genetic information and undoubtedly will continue to prove of great value in this area. Further, they may help us understand what factors determine whether cells in culture continue to carry out the specialized functions associated with tissues in vivo. The establishment of serially propagated, functioning animal cell cultures will open up broad new fields of wide significance to scientists in many disciplines.

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Nuclear Power Development in the United States

Government and industry are engaged in a joint effort to achieve economically competitive power by 1968.

Frank K. Pittman

There have been times when responsible individuals have seriously questioned the need for further development of atomic energy for the production of power. These attitudes have been motivated by economics, when it has been difficult to reconcile what appears to be an overabundance of relatively low-cost fossil fuels with costs of nuclear power development.

It is true that these costs have been high. Government and industry are spending about $200 million a year on civilian reactor development alone. However, the development program is directed toward building a new industry, achieving nuclear power production economically competitive with power from fossil fuels in areas where fossil fuels are costly (35 cents per 1000 Btu), and, subsequently, making nuclear power production economically competitive in more and more locations and with an ever-increasing range of plant sizes.

Significant advances have been made, but substantial problems remain to be solved before our objectives are achieved.

At the present time nuclear power is produced in custom-built, complex plants that are costly to build and operate because of their requirements for special fuels, materials, safety, and technology. The tremendous capital costs make it necessary for utilities to amortize nuclear stations as base-load power sources over the lifetime of the plants to achieve acceptable power costs. This is not a problem usually faced by utilities when they build and operate fossil-fueled power stations.

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According to all predictions, the recent tremendous rate of growth in the demand for electricity in the United States will continue. It has been estimated that by 1980 our present generating capacity of about 175 million kilowatts will have increased to approximately 465 million kilowatts, and that in the United States about 2235 billion net kilowatt-hours of electricity will be produced in 1980 as compared with current production of about 830 billion. Those who have made a careful study of our fossil-fuel resources say that our fuel supply is undoubtedly adequate to meet this predicted growth. They further say that any increase in the delivered cost of fossil fuel in the future probably will be largely offset by the tendency toward construction of larger generating units and by an expected continued decrease in the number of Btu's consumed for each kilowatt produced. For example, two units of 500 megawatt-electrical (MWE) capacity are currently in operation, and units of 800-MWE capacity are being built by the industry. The capital cost factor in plants such as these can be appreciably less than that in plants which are standard today.

Potential of Nuclear Power

Such competition is indeed formidable. It means that to make nuclear power competitive in the United States we must take full advantage of all the engineering and mechanical know-how of our science and industry to simplify design, to decrease construction costs, to increase thermal efficiencies, to make maximum use of fuel, and to minimize operation and maintenance costs. This job will not be accomplished overnight with the construction of a few experimental and prototype plants. It will only have been started when more power stations have been built and have reached equilibrium. Nevertheless, we are convinced that nuclear power can be a major factor in meeting the new generating requirements of the United States, and our nuclear development program stems from this conviction.

The key to progress in nuclear power production is improved and demonstrated technology. We did not even know that some of our problems existed until atomic fission and turbogenerating equipment were first united for power production in 1951. There was no need for materials capable of operating in an environment of extremely high temperature, pressure, and radiation prior to the era of nuclear power, and without this need, vast areas of materials technology remained unexplored. Therefore, the emphasis of our program has been on the development of this technology, which can enable us to achieve increased capability in the generation of nuclear power.

Concurrently with our development of materials we have had to determine the technical feasibility of various reactors and, after verifying this, determine the economics of each.

In order to appreciate the extent of the Atomic Energy Commission's development program, let us review some of the more significant aspects.

The development program is carried out in Commission, educational, and private laboratories and through construction and operation of experimental and prototype reactors.

Several reactor systems that appear to offer promise of producing economically competitive nuclear power are being examined. When classified according to coolants, the major reactor systems can be identified as light-water-cooled reactors (these include pressurized and boiling-water systems) and organic-cooled, sodium-cooled, gas-cooled, and heavy-water-cooled reactors.

Light-Water Reactors

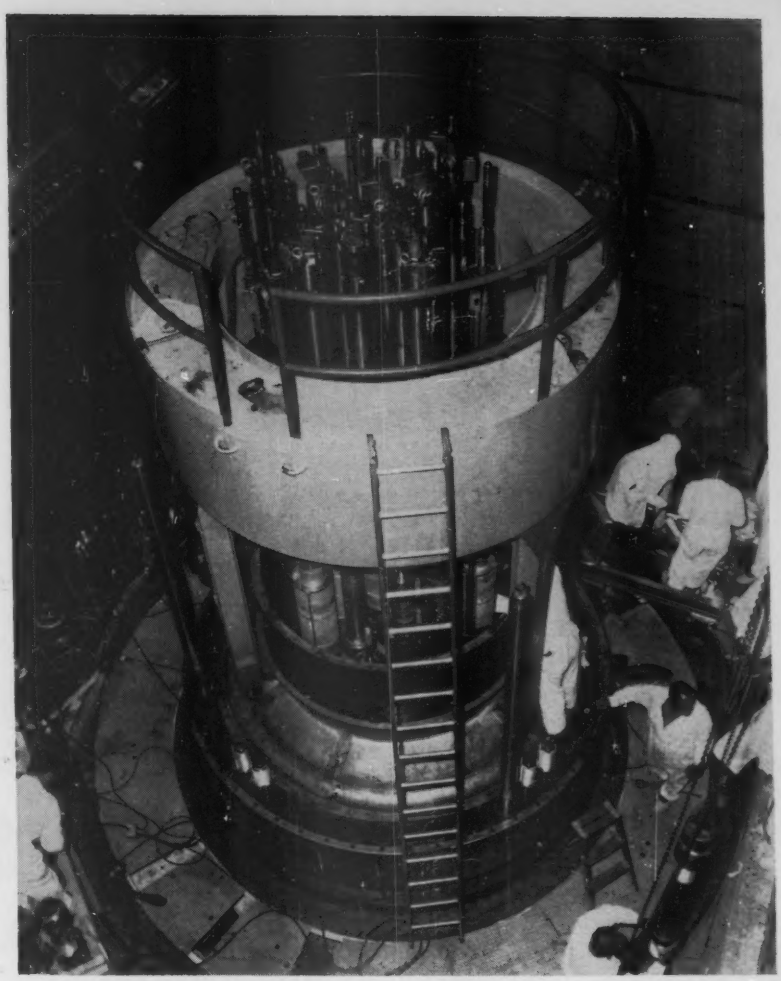
Reactors cooled and moderated with light water, fueled with slightly enriched uranium in the form of UO_2 clad in stainless steel or zirconium, and producing saturated steam for the turbines

are the type farthest developed technologically in the United States. Therefore, these systems seem the most promising for meeting our objective of producing economically competitive nuclear power in high-cost fuel areas by 1968. In the development program for these systems we are critically examining possible alternative reactor components and fuels in an effort to achieve further reductions in costs.

Pressurized-Water and Boiling-Water Systems

For the pressurized-water system, we are obtaining significant data on fuels, such as data on the lifetime-reactivity burnup of the enriched uranium seed and natural uranium blanket core, from operation of the Shippingport (Pennsylvania) Atomic Power Station. The 110-MWE Yankee Atomic Electric Company plant at Rowe, Massachusetts, with a pressurized-water reactor that achieved criticality 19 August 1960, will provide information on the use of slightly enriched fuel clad in stainless steel. The 255-MWE plant of the Consolidated Edison Company at Indian Point, New York, which is scheduled to become operational this year, with a thorium-U^{235} fuel mixture, will provide additional knowledge about reactor fuels. The Indian Point Station has a 151-MWE reactor and a 104-MWE oil-fired superheater. During 1961 the Saxton nuclear experimental reactor at Saxton, Pennsylvania, will begin operating and will provide information on higher specific power and heat flux, boiling of the coolant in the core, and use of dissolved poison for shim control. Later, if our discussions with utilities are successful, we will obtain information through the construction and operation of larger pressurized-water plants. Operation of plants of 300-MWE capacity or more could establish the validity of our assumption that, with current pressurized-water reactor technology, lower nuclear power costs can be achieved most readily with large plants.

We are also examining a spectral-shift reactor which has the basic characteristics of the pressurized-water system. This reactor uses a variable mixture of heavy and light water as moderator and coolant and has the potential advantage of providing more even power distribution, resulting in operation at higher power levels with higher average fuel burnups, and in high conversion ratios.



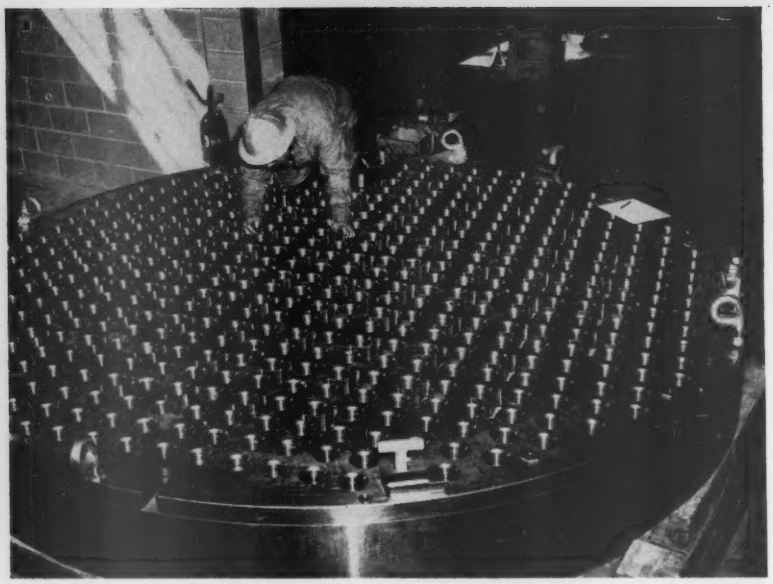
Yankee Atomic Electric Company reactor vessel head in place, 9 August 1960.

Where there are unusual operating or logistic conditions, use of small reactors can be justified, and our civilian reactor development program is reinforced by programs to develop reactors for special purposes. Data are obtained from our program (with the Army) to develop small-size reactors. We also obtain information applicable to the development of central nuclear power stations from our program (with the Navy) to develop nuclear power plants for submarines and surface ships, from our program (with the National Aeronautics and Space Administration) for developing nuclear rocket engines, from our efforts (with the Maritime Commission) to produce reactors for the propulsion of merchant ships, and very importantly from our program (with the Air Force, NASA, the Navy, and others) to develop systems to produce nuclear auxiliary power for space and other needs.

Much of our work on the boiling-water system involves simplifying design, reducing fuel costs, increasing the power density of the core, and improving the vapor-containment techniques.

Much of the development program for the resolution of these problems has been conducted with the Commission's experimental boiling-water reactor at Argonne National Laboratory, the boiling-reactor experiments at the National Reactor Testing Station in Idaho, and the privately owned Vallecitos boiling-water reactor at Pleasanton, California.

In addition, nuclear power stations that use boiling-water reactors have been, and are being, built by utilities



Bottom core support plate of the reactor pressure vessel is inspected for tolerances at the Dresden Nuclear Power Station, 23 March 1959.

and by the Atomic Energy Commission to produce electricity for commercial power systems. These operations will produce economic data which can be as important as the technical data obtained from reactor experiments. The 180-MWE Dresden station of the Commonwealth Edison Company, near Chicago, and the 22-MWE Elk River (Minnesota) plant are examples of such stations.

The Dresden reactor began operating last year but was shut down when stress cracks were discovered in some of the control-rod drive index tubes. Replacement parts are being fabricated, and resumption of operation is expected by the time this article appears in print.

The Elk River plant will soon be completed. It has an indirect-cycle boiling-water reactor plus a coal-fired superheater, and it will provide experience in the use of thorium oxide and uranium oxide fuels and of an intermediate heat exchanger in the boiling system.

Construction was started on the 50-MWE high-power-density reactor by the Consumers Power Company of Michigan in May 1960. The plant is scheduled to achieve criticality in the fall of 1962 and will provide technical and economic information on operation at power densities up to 60 kilowatts per liter, with fuel lifetimes and fuel fabrication costs similar to those achieved at lower densities.

Construction was also started last year on a 48.5-MWE plant at Humboldt Bay, near Eureka, California. This Pacific Gas and Electric Company plant is to be completed in late 1962 and will be the first to use a new pressure suppression and containment system in which vapor from a reactor accident would be expelled through a pool of water and pressure would be reduced by condensation. This may remove the need, in some cases, for a pressure containment building, further reducing capital and power costs.

Nuclear Superheating

Another method of reducing power costs is through the use of superheating. Superheating makes possible the production of steam by nuclear reactors at temperature and pressure conditions found to be most efficient in modern generating equipment. Oil- or coal-fired superheaters, such as that used at the Elk River plant, can be used, but we are especially interested in *nuclear* superheating.

The desirability of using nuclear superheating varies with the size of the unit. In small reactors, superheating appears to be of more value in the direct-cycle than in the indirect-cycle reactors. Although superheating is applicable to both boiling- and pressurized-water reactors, it appears to be of most economic benefit when used with direct-cycle boiling-water reactors.

As part of our effort to develop nuclear superheating, we are initiating critical experiments and conducting tests of heat transfer, steam separation, corrosion-erosion, and steam purity. In addition, three plants are now under construction which will examine integral nuclear superheating arrangements. One is the government's boiling-reactor experiment No. 5, BORAX V, scheduled to begin in mid-1961, to provide data on superheating and on forced circulation and various core configurations for the further development of boiling-water reactors. The other plants are the Pathfinder atomic power plant at Sioux Falls, South Dakota, and the boiling nuclear superheat reactor, called BONUS, at Punta Higuera in Puerto Rico. Pathfinder will have a superheater that is centrally located with respect to the boiling-core region. It is scheduled to achieve criticality in mid-1962. BONUS, which will have a peripheral superheating region, is scheduled to be in operation in early 1963.

The Commission is also investigating the use of separate superheating reactors.

A group of utilities recently announced the financing of a development program which could result in the design and construction of a large nuclear plant. Use of a nuclear superheating reactor is under consideration. If the development effort is successful and a nuclear superheating reactor is chosen, this will encourage other utilities to consider using nuclear superheaters when they decide to construct nuclear power stations.

Organic Systems

The organic-cooled and moderated reactor is similar in many respects to the water system but offers the additional advantages of operating at low pressure and of presenting fewer corrosion problems. Its advantages are somewhat offset by the fact that the organic materials now available are polymerized by radiation and must be continuously replaced. However, with construction of a 50- to 75-MWE organic prototype reactor, to be initiated late this year or early in 1962, and the recent development of a new, improved fuel system, there is every reason to be-

lieve that the organic-cooled and moderated system will be capable of meeting our program objective of production of economically competitive nuclear power by 1968.

The new fuel system, which uses UO_2 as the fuel material capable of long exposure, is clad in an aluminum–aluminum oxide cermet. This fuel has satisfactory strength and heat-transfer characteristics at high temperatures.

While this new fuel was being successfully developed, there was a build-up of film on some of the fuel elements in the Commission's organic-moderated reactor experiment (OMRE), which resulted in partial blocking of fuel-element cooling channels. The film is believed to have been caused by coolant decomposition and by inorganic particulate matter in the coolant. The reactor is being modified to correct the problem. Much of our development of the system to date has been through operation of this reactor. We are building an experimental organic-cooled reactor (EOCR) to complement the work of this facility.

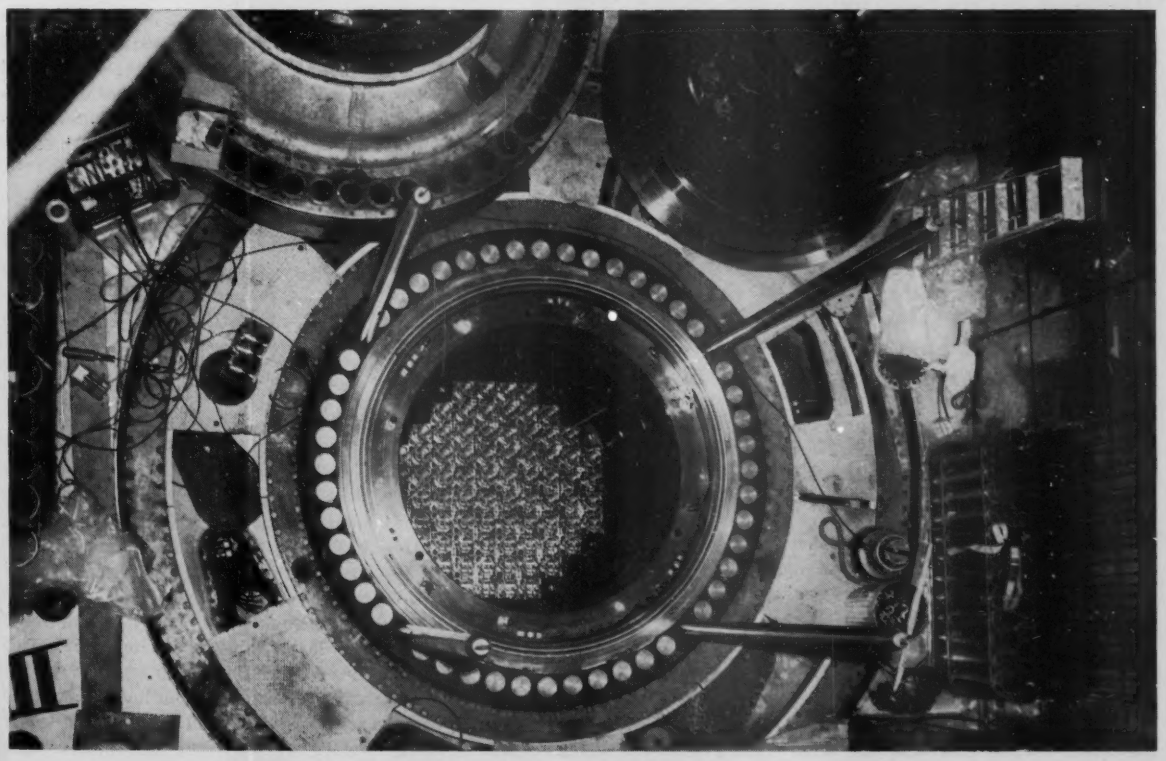
The first operating organic-cooled reactor to be incorporated in a utility power system is that in the Piqua plant at Piqua, Ohio. This 11.4-MWE reactor is scheduled to go into operation late this year. Its operation will demonstrate the technical and economic feasibility of using small organic reactors in nuclear power stations.

Use of the new fuel in a larger prototype will demonstrate the ability of organic reactors to meet the short-range objective—production of economically competitive power by 1968. It is expected that operation of this new prototype and of the OMRE, the EOCR, and the Piqua plant will show that this system is also capable of subsequently achieving our long-range objective—economical production of nuclear power.

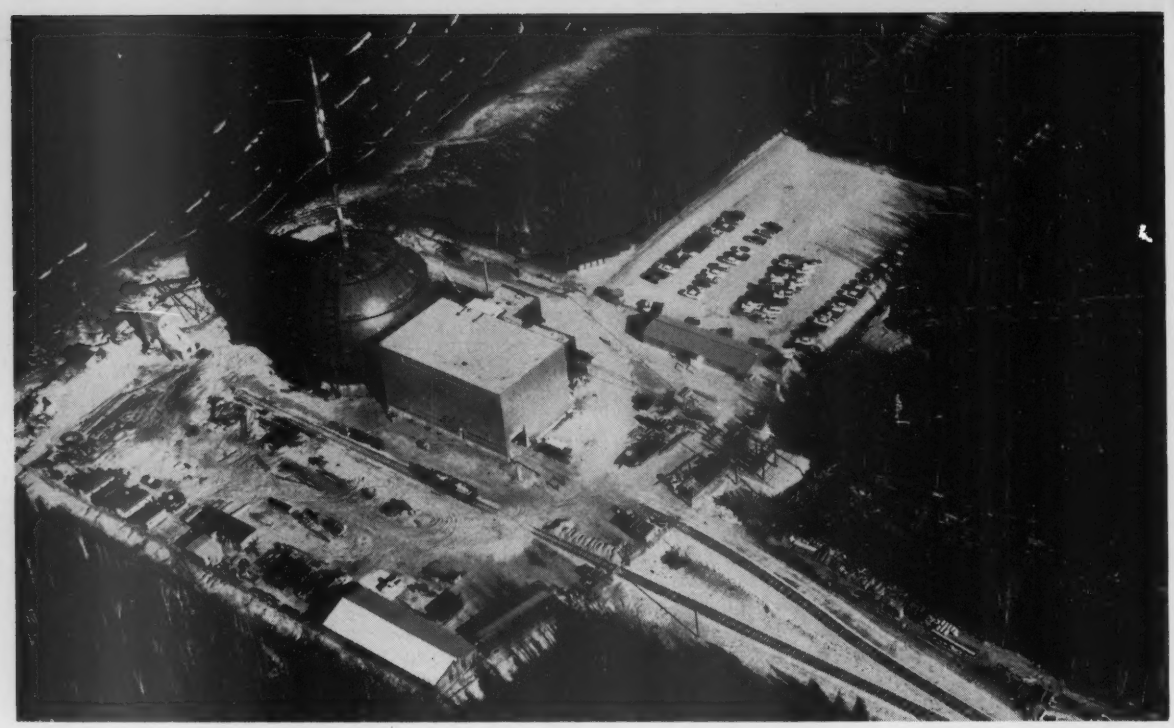
Sodium-Cooled Systems

Liquid sodium is the metal that seems most promising as a power reactor coolant at the present time. Sodium-cooled reactors can operate over a wide spectrum of neutron energies, from fast to thermal, depending upon the design characteristics of the core. Another attractive feature of such reactors is their use, at relatively low pressures, of a liquid which has a high boiling point, reasonably low neutron absorption, and excellent heat-transfer and heat-transport capabilities. These features result in very high plant efficiency, and although reactors cooled by liquid metal may not be competitive by 1968, they are expected to meet our long-range objective.

We have used the Atomic Energy Commission's experimental breeder reactor No. 1 (EBR-1) to investigate fast-reactor stability, and we will use EBR-2 when it is completed, late this year, to demonstrate the engineering feasibility of using a fast reactor for power generation. In addition, two nuclear power stations, the 94-MWE Enrico Fermi plant at Monroe, Michigan, and the 75-MWE Hallam plant at Hallam, Nebraska, are under construction. They will provide significant operating data, which can be integrated with that obtained from other sodium-cooled facilities to help us determine what research and development effort should be made for this system. However, just as variations of the other concepts are being studied,



Yankee Atomic Electric Company core loaded in the reactor, 29 July 1960.



Consumers Power Company's Big Rock Point plant under construction, 23 November 1960.

variations of the basic liquid-metal system are being examined. For example, studies are being made of an advanced epithermal reactor which, at this stage of development, appears capable of retaining the high thermal efficiency and low pressures of a sodium-cooled system while achieving a high conversion ratio with uranium-233 as fuel. The studies on this particular reactor are being conducted by the Commission and by private industry.

We are also obtaining information about sodium-cooled fast reactors from foreign countries. We are engaged in a cooperative research and development program with the United Kingdom, and our programs are coordinated to avoid duplication of effort. The French have made progress with their program, and we hope to expand our collaboration with them.

Gas-Cooled Systems

In the gas-cooled reactor development program, the major Commission effort is directed toward the design and construction of a 25-MWE experimental gas-cooled reactor scheduled for completion at Oak Ridge, Tennessee, next year and of a privately-owned 40-MWE high-temperature gas-cooled reactor to be constructed at Peach Bottom, Pennsylvania. These plants will provide information and experience with high-temperature helium systems, fuels, and components which are needed in the long-range effort to achieve gas-cooled power plants that operate economically.

Additional projects contributing to the development of gas-cooled reactor technology include examination of a pebble-bed reactor concept and a beryllium oxide experiment.

The pebble-bed reactor uses a stationary bed of spherical fuel bodies containing fissionable and fertile material in the form of coated particles (discussed in more detail below) which are dispersed in a graphite matrix. Cooling is accomplished by helium gas flowing through the bed. Potential advantages of this reactor are simplified fuel fabrication and handling, and high thermal efficiency.

Construction of a 10-MW (thermal) beryllium oxide experiment is being initiated to obtain basic engineering and physics data on beryllium-moderated, gas-cooled reactors.

As with the sodium-cooled system, the gas-cooled system is being developed through a cooperative arrangement with the United Kingdom. This is the power reactor system most used in Great Britain. In the United States the gas-cooled system is expected to meet the Commission's long-range objective by becoming economically competitive sometime after 1968.

In addition to all of these activities, the Army gas-cooled reactor experiment, which has been operating since February 1960, will continue to provide operating experience which will contribute to the development of gas-cooled reactors for the civilian economy.

Heavy-Water-Moderated Systems

One other major reactor system under development can help us achieve competitive nuclear power. This is the heavy-water-moderated system. The biggest advantage of this system is that natural uranium fuel can be used, dependence upon enriched uranium in the fuel cycle thus being removed. This makes natural-uranium heavy-water reactors especially important to countries not having diffusion plants or other means of providing enrichment. The biggest disadvantage is that natural-

uranium heavy-water reactors are physically large and that high capital costs are associated with the plant and with the heavy-water inventory. In addition, the reactivity lifetime of natural uranium is more limited than that of enriched uranium.

In the United States two heavy-water-moderated reactors are under construction, and another is in operation. Two of these reactors are test reactors located at Commission sites, one at Hanford (Washington) and the other at Aiken (South Carolina). The third, a plant being built by Carolinas-Virginia Nuclear Power Associates at Parr, South Carolina, is a 17-MWE nuclear power plant. All use enriched fuel, but the technology developed will be applicable to heavy-water-moderated reactors fueled with natural uranium, such as those in Canada.

Our program for developing heavy-water reactors is closely coupled with the Canadian program. As our contribution to the joint effort, we are conducting research and development work in this country, concentrating on developing improved methods for predicting reactivity lifetime; on means, including fuel-management techniques of extending the lifetime of fuels; on methods for minimizing heavy-water inventory and loss; and on improved techniques for component fabrication.

The arrangement with Canada has been designed to give us as much information as we could have obtained if we had built in the United States the plants that are being built in Canada.

Fluid-Fuel Systems

In addition to developing systems that give promise of attaining program objectives, we are investigating advanced technical and engineering concepts—for example, fluid-fuel reactors, which use fuels of molten plutonium and molten salt. These systems offer the potential advantages of high thermal efficiency, high power density and specific power, and simplified fuel processing.

We do not know whether a promising power-producing reactor can be developed from these or from other, more advanced, concepts, but engineering studies, research and development, and evaluation of the concepts will be continued to the point where a decision can be made either to proceed or to terminate our efforts.

Nuclear Technology Development Program

While working on specific reactor systems, the Atomic Energy Commission has set up programs to develop technology *generally* applicable to reactor systems and related operations. The objectives of this broad-based nuclear engineering and development program are to provide data on such matters as reactor fuels and materials, reactor physics, reactor components, reactor safety, and environmental and sanitary engineering, and to provide tools, such as test and research reactors and remote-handling devices, for use in our research and development effort. All of this work is important and significant results are being obtained in many areas.

A good example of the type of work carried out under this program is the research and development program on nuclear fuels and materials to determine the potential of fuels and materials for reactor applications, to define their basic properties, and to develop engineering and design information for reactor systems. The over-all objective is to reduce fuel-cycle costs through increasing the life of the reactor core and fuel burnup, increasing the irradiation stability of nuclear fuels, reducing fuel fabrication costs, and attaining operation of fuel assemblies at higher temperatures.

Vibratory-compaction techniques for fabricating the UO_2 fuel elements have been developed and demonstrated; 90 percent of the theoretical oxide density is readily attainable. Powdered fuel is inserted into a tube, and the powder is compacted by the application of cyclic forces. The tube then can become a fuel rod when placed in a reactor. Successful development of the vibratory-compaction technique will eliminate some present difficulties—those of obtaining uniform pellet density and of inspecting hundreds of pellets individually, and difficulties due to the extremely close tolerances between the fuels and their containing tubes. The new compaction technique appears to be particularly applicable to remote fabrication of "recycled" fuels. Increased amounts of fission products will be released from the fuel when it is processed by this technique. However, it is expected that this problem will be overcome and that the technique can be extended to the fabrication of thorium oxide and uranium carbide fuels.

Recent developments in the retention of fission products by spherical UO_2 particles coated with Al_2O_3 dispersed in graphite matrix fuel have been very encouraging. This development may eliminate the need for fuel cladding as we now know it in gas-cooled reactors.

In the "coated-particle" process, small particles of the fissionable nuclear fuel compound, such as uranium oxide or carbide, are coated individually with a dense, refractory material such as alumina or pyrolitic graphite. After coating, the fuel particles are evenly dispersed in a material, such as graphite, which can be shaped into reactor fuel elements by mass-production methods. This coating protects the fuel from damage by chemical reaction at high temperatures and prevents escape of the troublesome radioactive by-products formed in the fuel by the fission process.

Coated-particle fuels appear attractive for high-temperature operation because only ceramic materials are utilized. Since good neutron economy can be expected, coated-particle fuels should also be useful in low- and intermediate-temperature reactors.

Future Development in Fuels and Materials Research

Further research work in fuels and materials is being directed toward establishing more basic information concerning the alloys and ceramics of uranium, thorium, and plutonium. Intensified research work is required on the properties of materials at elevated temperatures and the determination of the effects of radiation on the properties and performance of reactor materials in reactor environments. The effects of long-term irradiation are of particular importance. Improved fuel-element fabrication methods will continue to be sought. Additional effort will be directed toward an understanding of the mechanisms of fission gas retention and of the behavior of oxide fuel elements under irradiation at temperatures which result in central melting or vapor-phase transfer of the fuel. Research on nondestructive testing techniques will continue, with special emphasis on development of improved testing equipment.

In our reactor development program we will continue to work to simplify design, to minimize maintenance, and to increase dependability of reactor

plant operation. We will be seeking ways to get more power from present fuels, to improve and simplify fuel-processing techniques, and to develop methods whereby radioactive wastes will be less of an economic burden.

Reactor safety will continue to be one of our most important areas of study, testing, and evaluation.

All of this will continue to require a relatively expensive program, but while it is true that development costs are high and that fossil fuels will meet our nation's power needs for many years to come, other factors, such as the needs of our national defense, the need to conserve our natural resources, and—also of major importance—the need for man to continue to explore the new frontiers of science and technology, will require the continued development of nuclear power. In addition, we will be developing a new and healthy industry which gradually will assume a more important role in our economy. As industry assumes more responsibility for nuclear power development, we will be able to turn to other areas of this new science—areas which require resources that only the government can supply.

Protection of Rainbow Bridge National Monument

An exchange of views on the effects of Glen Canyon dam shows that complex problems remain to be solved.

Comment by Halliday

The problem of protecting Rainbow Bridge National Monument from the waters of Glen Canyon reservoir is complex. Although there has been a 5-year period during which detailed studies could have been made—studies on which rational decisions might be based—available data on the subject are scanty, incomplete, and contradictory.

Many factors must be considered before acceptance of the drastic and irreversible step of abolishing, or abandoning by default, national-monument-type protection for Rainbow Bridge, as recently proposed by A. M. Woodbury in *Science* (*1*), and as proposed on other occasions by other supporters and by officials of the Bureau of Reclamation. Since the effects of this proposal would be irreversible, available data must be analyzed in detail, and certain alternative proposals which were ignored or summarily dismissed by Woodbury must be given due consideration. At the present time, such a "default decision," based on governmental inaction rather than on rational considerations, is imminent. As discussed below, the impending filling of Glen Canyon reservoir now threatens Rainbow Bridge National Monument (*2*, *3*) despite its supposed legal protection (*4*). The filling of the reservoir would also provide a precedent for the construction of Echo Park dam in Dinosaur National Monument, a recently revived project, and for construction of other dams which have been proposed in locations which would adversely affect Yellowstone National Park, Grand Canyon National Monument, Glacier National Park, and other units of the National Park System. In evaluating Woodbury's article on Rainbow Bridge it should be remembered that in 1954, when the Bureau of Reclamation was struggling for approval of the Upper Colorado Storage Project Act, Woodbury similarly advocated construction of Echo Park dam in Dinosaur National Monument, in two articles in *Science* (*5*, *6*), and dismissed as of little importance "whether we are setting a precedent of invading a national monument, *and various other minor matters*" (italics mine) (*5*).

Many discrepancies on both vital and trivial matters in reports and public statements of the Bureau of Reclamation make it difficult to conduct a precise analysis of this matter. In one official report, for example, the distance from Rainbow Bridge to the Colorado River is variously given as 6 miles and 4½ miles (*7*). In 1957 it was stated that the surface of the reservoir would be at 3700 feet 7 percent of the time (*8*). In 1959 and 1960 (*1*, *7*), the figure was given as 13 percent. An official 1954 "Fact Sheet" of the Department of the Interior not only used an incorrect name for the national monument but erroneously stated that it was threatened by the San Juan River arm of the reservoir, and that the monument could be protected by a mere "dike" (*9*). These and similar errors and inconsistencies which have come to light during study of this problem contrast remarkably with the professional reputation of the Bureau of Reclamation. However, it does appear, upon careful study of available data, that enough information is available to permit considered action—and to indicate that it is needed in the immediate future.

Basic Geographic Factors

Rainbow Bridge National Monument (Fig. 1) is located in the slick-rock country of south-central Utah, about five miles north of the Arizona-Utah state line, in the magnificent Glen Canyon area. The monument encompasses 160 acres on the north fork (Bridge Canyon) of a tributary canyon (Aztec Canyon) of the Colorado River's Glen Canyon section. Bridge Canyon is spanned by Rainbow Bridge.

Because of the length and difficulty of the trails from the nearest road ends, most of the 2000-odd annual visitors to Rainbow Bridge National Monument (*10*) now use the river route. In linear distance, the monument is about 2¼ miles from the Colorado River, but the gentle trail up Aztec Canyon and Bridge Canyon is about 4¾ miles long, as determined by Bureau of Reclamation surveys. Rainbow Bridge itself spans an inner gorge of Bridge Canyon, which will be completely filled at high water of the reservoir if no barrier dam is erected.

Woodbury was in error in statements about the maximum height of the reservoir and hence about the proximity of the reservoir to the base of

Rainbow Bridge. The maximum height of the reservoir will be at 3715 feet (*7*, *11*) rather than at 3700 feet, as stated (*1*). The 3700-foot level, rather than the maximum, is to be the "normal surface elevation" of the reservoir. It has been stated that part of the additional capacity will be used very rarely (*8*). However, in the spring of 1941, Roosevelt Dam, another large Bureau of Reclamation dam in Arizona, operated at more than its stated maximum storage capacity for several weeks. Conditions requiring a similar occurrence must be anticipated in Glen Canyon and in all of its tributaries.

With the level at 3715 feet, the reservoir would temporarily rise to within 6 feet of one buttress of Rainbow Bridge, which is at 3721 feet (*7*, *8*), rather than to within "40 to 50 feet," as claimed by Woodbury. The inner channel beneath the great arch would be filled with water, silt, and debris 55 feet deep (Fig. 2).

As now planned, the reservoir will begin to fill in January 1962 (*2*), and water will be allowed to rise to the dead storage level (elevation, 3490 feet) "at the earliest practical time" (*2*). This level will be 125 feet above the elevation of the only satisfactory site for a restraining dam to keep the reservoir from encroaching on the national monument. Such a dam would require at least one year to build, and perhaps much more. For the reservoir to rise to the 3490-foot level would require about 6½ million acre-feet of water. The average annual flow of the Colorado River is almost twice this amount, and the annual flow at times has exceeded 24 million acre-feet per year (*12*). Most of this flow results from the seasonal runoff in late spring, so that even if the flow of the Colorado River is below average, default on construction of this dam would constitute an irrevocable "decision" by the summer of 1962.

Upstream from Rainbow Bridge the canyon floor gradually ascends to reach the 3715-foot level a short distance above the national monument. The downstream boundary is just above the minimum level planned for the reservoir during its first 50 years of operation. As a result, the elevation of the zone of fluctuation of the reservoir corresponds closely with the elevation of the canyon bottom within the monument. Since it is within the zone of fluctuation of similar reservoirs that severe damage has occurred, this is a key factor in analysis of the problem.

History of Pertinent Legislation

A knowledge of certain legislative actions pertinent to this area is essential to an understanding of the present problem. In 1956 the Colorado Storage Project Act became law (*4*). As a part of this act, the construction of a 700-foot dam in Glen Canyon, just south of the Utah-Arizona border, was authorized, provided that "as a part of the Glen Canyon unit, the Secretary of the Interior shall take adequate protective measures to preclude the impairment of the Rainbow Bridge National Monument." It was further stated that "it is the intention of Congress that no dam nor reservoir constructed under the authorization of this act shall be within any national park or monument."

These provisions were introduced into this bill as a result of an agreement between proponents of the Upper Colorado Storage Project and representatives of conservation organizations (*13*). The provisions were included when it became apparent that the mobilization of public opinion by the conservation organizations for the purpose of protecting Echo Park in Dinosaur National Monument, Rainbow Bridge National Monument, and other units of the National Park System had been successful (*13*). Had a vote been taken on the original proposal, which did not provide this protection, the bill would have been defeated (*14*), on the grounds that passage would have resulted in damage to these areas and would have established a precedent for invasion of other national parks and monuments.

At that time, a spokesman for the Bureau of Reclamation stated: "We can build the necessary works [below Rainbow Bridge] to protect that bridge in the manner suitable to the National Park Service and others that are interested, within the amounts of money that we have estimated in our overall estimate for the Glen Canyon Dam and Reservoir, and we have no question about the economic, engineering or

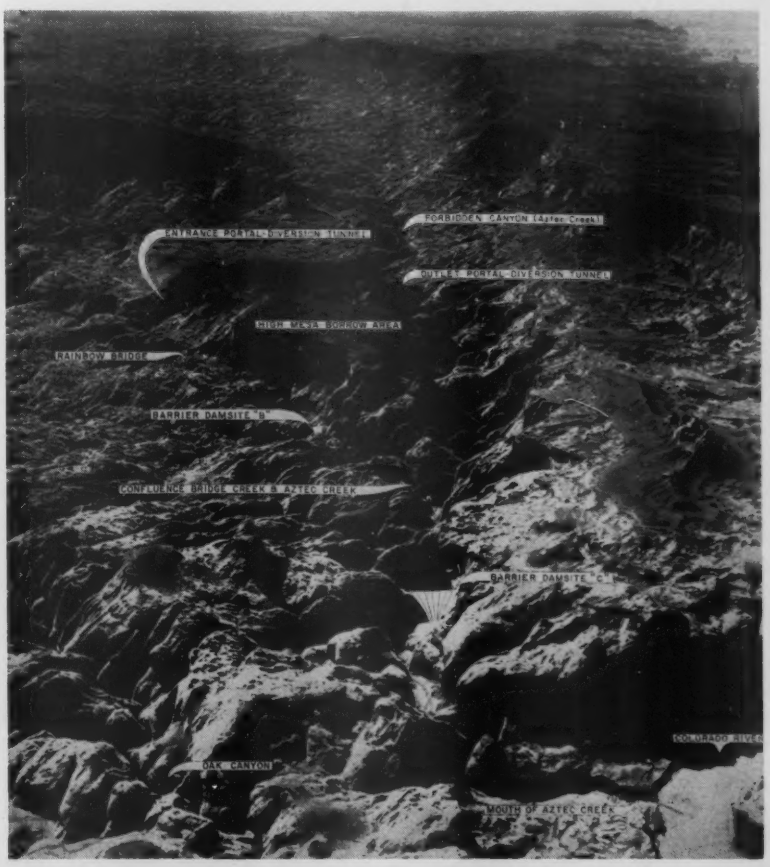


Fig. 1. Aerial photograph of the region around Rainbow Bridge, looking south. [U.S. Bureau of Reclamation]

practical feasibility of taking care of that monument" (*15*).

The plan to build protective works seems to have been rescinded. Reclamation spokesmen have repeatedly emphasized, in recent statements, the difficulty of constructing such works, and have claimed that they are unnecessary (*16*).

Possible Courses of Action

There appear to be various possible courses of action in regard to the threatened inundation of Rainbow Bridge National Monument. Only three of these (1, 5, and 9, below) were considered by Woodbury in the recent article in *Science* (*1*).

1) Allow the reservoir to invade the national monument.

2) Abandon the Glen Canyon dam project.

3) Allow the reservoir to invade the national monument to determine how much damage it would do, before taking steps for protection.

4) Enlarge Rainbow Bridge National Monument to such an extent that it could not be protected from damage from the reservoir and hence would require abandonment of the Glen Canyon dam project under present law.

5) Abolish Rainbow Bridge National Monument.

6) Reduce the height of Glen Canyon dam.

7) Reduce the depth of dead storage space in the reservoir.

8) Delay construction of Glen Canyon dam.

9) Construct the proposed dam and also a barrier dam at "site *B*."

10) Construct the proposed barrier dam at "site *C*."

Some of these plans can be bypassed with only a few words of explanation. The proposal to allow temporary inundation within the monument to determine the resulting damage seems to have been abandoned by its originators. It is contrary to the National Parks Act of 1916 (*17*), to the Colorado Storage Project Act (*4*), and to basic principles of the National Park System and of leading conservation organizations; it would require a very difficult legislative campaign at some indefinite future date; and, finally, it is eliminated by the finding that, because of the peculiar design of Glen Canyon dam, once the reservoir was about one-fourth full, the water level could not again be lowered sufficiently to allow the site for the proposed protective project to reemerge. The idea of enlarging Rainbow Bridge National Monument to force abandonment of the Glen Canyon dam project, while ingenious, is too far-fetched and smacks too much of trickery to be considered seriously. However, enlargement of the monument when it has been fully protected, in such a way that there will be no interference between the monument and the reservoir, as discussed below, is both practical and desirable. Abolition of Rainbow Bridge National Monument would solve the problem from the standpoint of reclamation enthusiasts, but it is doubtful that the American people would knowingly allow such a fate to befall this uniquely magnificent, world-famous monument. On the other hand, if through a "default decision" the monument is invaded by the reservoir, abolition of the monument probably will be necessary for various legal and administrative reasons. The proposals for reducing the height of Glen Canyon dam, for delaying its completion, and for redesigning the dam to permit a lower water level for the reservoir are better comprehended when discussed with other proposals, as they are below.

Invasion of the National Monument

Much has been said and written in support of the idea that flooding and aggradation in Rainbow Bridge National Monument would "enhance" rather than impair it (*1*, *16*). [Only one of the individuals who hold this view (*1*) has conceded that "for probably 50 or 100 years" the zone of fluctuation might be of less than prime scenic quality.] Analysis of information obtained from the existing, similar reservoir of Lake Mead, together with personal investigations of these areas and study of applicable legal and related data, has led me to a conclusion opposite to that of Woodbury in his recent, undocumented article in *Science* (*1*).

In addition to the fact that artificial flooding and aggradation of this sort would be contrary to law and to the principles of the National Park System (*17*), there is considerable evidence that flooding and aggradation of sediments, sand, and silt in and near Rainbow Bridge National Monument would be highly detrimental to that monument and the adjoining area and that the proximity of the reservoir would seriously threaten the stability of Rainbow Bridge itself.

In filling the inner gorge of Bridge Canyon to a depth of 55 feet beneath Rainbow Bridge, and to a much greater depth a short distance downstream, the reservoir would saturate the upper part of the Kayenta formation on which the Navajo sandstone of the bridge rests (*11*). This upper part of the Kayenta formation is "less resistant to erosion than the lower part" (*18*) and hence a matter of concern in the zone of fluctuation. The last official statement of the National Park Service on this subject expressed great concern about this matter, and the Park Service requested a reduction in the height of Glen Canyon dam for that reason (*19*). This reduction was not achieved, probably because of the statement of the Bureau of Reclamation quoted above (*15*) and because of subsequent reports by some Interior Department geologists which are in conflict with National Park Service reports. It has been stated that the Kayenta formation beneath Rainbow Bridge is saturated under present conditions and that consequently flooding and aggradation would retard rather than hasten collapse of Rainbow Bridge (*7*). This is partially implied in the oft-quoted final section of a U.S. Geological Survey report (*11*), but not in the body of that report, which, instead, mentions the presence of *local* saturation and perched water tables in the canyon walls beneath the bridge, with the water table at or just below the present level of the creek. This was also my conclusion when I made a short field study in 1953.

These conflicting conclusions are cause for serious concern for the safety of Rainbow Bridge if flooding of Bridge Canyon is permitted. Bureau of Reclamation studies indicate that standing water would be present beneath Rainbow Bridge 77 percent of the time in the absence of a restraining dam (*7*). Any possible error in efforts to resolve these conflicting opinions must be in the direction of greater safety of the bridge. If those who fear that the bridge will collapse are mistaken and yet prevail, the error will be of little significance. If those who hold the reverse view prevail, and are mistaken, and the reservoir causes the collapse of Rainbow Bridge, the error will be grave beyond words.

A very important additional factor, not mentioned in any published state-

ment on this problem, is the possible occurrence of pseudokarst in the Rainbow Bridge area. Its occurrence a few miles to the north in a region of similar stratigraphy has been known to staff members of the U.S. Geological Survey for several years (*20*), but until mid-1960 the Bureau of Reclamation apparently was not aware of its presence. Until the cause of this nearby phenomenon and the occurrence or nonoccurrence of pseudokarst in the Rainbow Bridge area have been determined, pseudokarst must be considered at least a potential danger to Rainbow Bridge and the reservoir must be kept as far away from the bridge as possible (*21*). Here again, the wrong decision could lead to disaster.

As mentioned above, about 95 percent of the zone of fluctuation of the arm of the reservoir that extends through the monument would be within the monument boundaries (*7*, *11*). This zone is the site of heaviest aggradation within such reservoirs. From the examples of similar tributary canyons of Lake Mead, where very large accumulations of silt, sand, quicksand, and drifted debris have developed, the conditions that would result from sediments in Rainbow Bridge National Monument can be deduced. It is of some significance that one senatorial proponent of the Colorado Storage Project has an intimate knowledge of the Rainbow Bridge area. He states regretfully (*22*): "Of far more concern to me than the dangers to [Rainbow] Bridge from water is the fact that after one or two seasons of floods, Forbidden Canyon [the combined term for Aztec and Bridge Canyons] will be actually just that. I can predict this with accuracy because the side canyons entering Lake Mead below Separation Canyon are now [1954] clogged with silt. This likewise will happen to all of the side canyons of Glen Canyon."

Many persons who are familiar with the Glen Canyon area concur in the belief that a sewer-like swamp of quicksand, silt, and impenetrable vegetation may block access to Rainbow Bridge from the reservoir if no protective structure is provided. They cite the example of side canyons of the Colorado River in which the profiles of the creeks now bear approximately the same grade relationship to the Colorado River that Bridge Creek will bear to the reservoir. Chamber-of-commerce type statements about anticipated wonders of the reservoir as a whole, although given wide credence, seem overstated. For example, a recent study (*23*) indicates that serious fishery problems may attend the creation of the reservoir. Many persons, perhaps including Woodbury, have been misinformed about the long-term recreational values of Lake Mead, and hence of the Glen Canyon reservoir. Figures on visitors to the Lake Mead Recreation Area, which are largely based on the number of cars that cross Hoover (Boulder) Dam en route to Las Vegas, are misleading. Besides blockage of many sections of the lake by aggradation, which caused abandonment of the plans for extensive development at Pierce's Ferry, a serious problem of spontaneous bacterial accumulation in lake sediments has forced closure of many areas to the public. The U.S. Geological Survey has compared these contaminated sediments unfavorably with raw sewage (*24*). Bacterial counts exceeded 1 million bacteria per gram at all depths, with a minimum of 10 million bacteria per gram near the surface. A similar sewer-like result must be anticipated in the Glen Canyon reservoir, including its Aztec–Bridge Canyon arm.

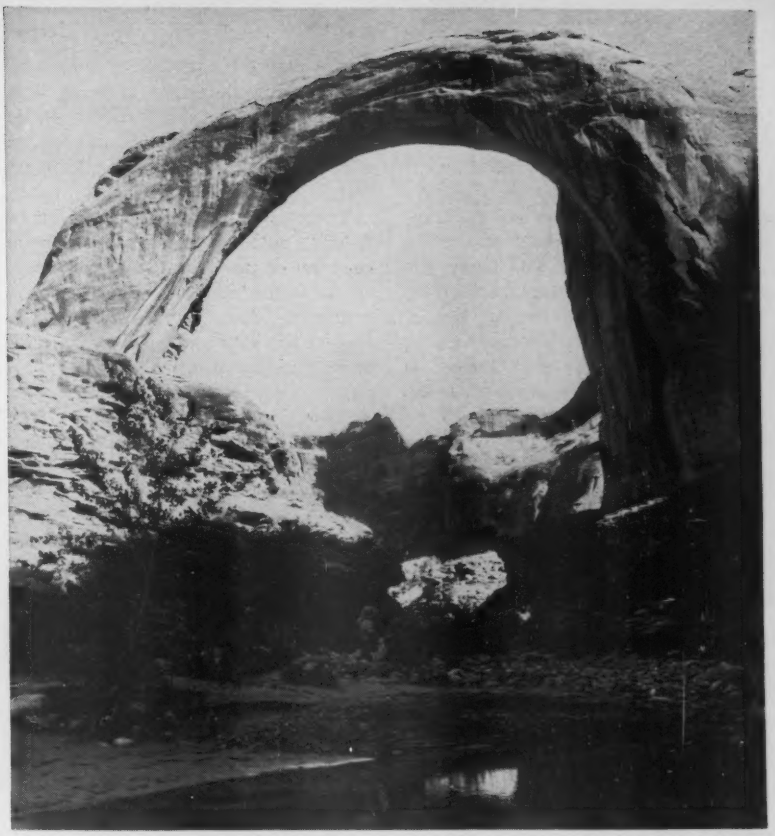


Fig. 2. View of Rainbow Bridge from an ephemeral pool in Bridge Canyon. Silts, gravels, and cobbles primarily deposited and transported by flash floods are shown. At present, high-velocity flow prevents massive aggradation of these deposits. In the absence of a downstream barrier dam the reservoir will fill this inner gorge completely and will rise to a point only 6 feet below the buttress at right. [Walter S. Chamberlin]

Figures 3 and 4, showing the zone of fluctuation of Lake Mead, indicate a part of what can be expected for much of the Glen Canyon reservoir within a very few years. Woodbury's recent condemnation of conservationists for "joining a crusade" (*1*) to prevent this fate for Rainbow Bridge National Monument seems unjustified.

It therefore appears that future proximity of the Glen Canyon reservoir may seriously threaten the stability of Rainbow Bridge. Pertinent data are incomplete and contradictory. On the other hand, evidence that the proposed flooding and aggradation within the monument would drastically impair it is overwhelming, and such flooding would and should be illegal.

Abandonment of Glen Canyon Dam

The opposite course to abandoning Rainbow Bridge National Monument would be to abandon construction of the partially built Glen Canyon dam. Although many conservation factors suggest that this would be the ideal solution from some standpoints, drawbacks to such a proposal appear insuperable at this time. Too many millions of dollars have been spent on the beginnings of this extremely costly dam to permit its abandonment except for the most compelling reasons. It appears that another alternative will permit both establishment of the reservoir and full protection of the monument. Therefore, unless for some reason the protective project discussed below cannot be built, compelling reasons for abandoning construction of the dam cannot be said to exist.

Barrier-Dam Proposals

If water and sediments resulting from the construction of Glen Canyon reservoir are not to be permitted within Rainbow Bridge National Monument, either a barrier dam must be constructed downstream from the monument or the height of Glen Canyon dam must be lowered drastically. As discussed below, the latter alternative does not seem practical. Four sites for a barrier dam between Rainbow Bridge and the Colorado River have been proposed. One of these sites, at the Narrows of Bridge Canyon, a few feet upstream from its confluence with Aztec Canyon, is impractical for engineering reasons (*7*). Another, site *A* in Bridge Canyon, would have all the disadvantages of site *B*, a short distance farther downstream, and a dam at site *A* would have even less capacity to withstand flash floods than one at site *B*. Site *A* is no longer under consideration. Woodbury discussed in detail (*1*) the project which includes a restraining dam at site *B*, but I will give some additional information. Woodbury dismissed the proposal concerning site *C*, in Aztec Canyon, three miles downstream from the monument, as if this plan were not worthy of consideration. Actually, it is site *C* which has been acclaimed by leading conservation spokesmen, not site *B*. It is difficult to understand an error of such magnitude in a supposedly authoritative article.

The Site B Project

The plan for a protective project involving a restraining dam at site *B*, 3200 feet downstream from Rainbow Bridge National Monument, would also require a diversion tunnel and barrier upstream from the monument, to divert water, sediments, and flood debris from upper Bridge Canyon to upper Aztec Canyon because of the very limited storage space on the upstream side of such a restraining dam. As a result of this analysis, it appears that Woodbury is correct in opposing this project (*1*), although some of his objections were overstated. Use of the "High Mesa Borrow Area," for example, would not necessarily be an integral part of such a project; the diversion tunnel could be drilled from Aztec to Bridge Canyon rather than in the opposite direction, and many of the construction scars would be submerged. But other factors, including inadequate upstream reservoir space, uncertainty of siltation rate, headward aggradation into the national monument (*11*), and the threat from possible pseudokarst, discussed above, make site *B* unsuitable for a restraining dam, even though this is the plan favored by the Bureau of Reclamation if any protective project must be built.

Reservoir storage upstream from site *B*, for example, is sufficient to handle local runoff from only one 5-hour flash flood per 30 days (*7*). Most individuals who are familiar with the erratic timing of rainstorms in this "slickrock country" would consider this completely inadequate in view of the absence of data to the contrary.

Another example of unacceptable planning for this site is the calculation of the rate of sedimentation behind the restraining dam, which supposedly indicates that sediment storage space will be adequate until the year A.D. 2140 (*1*). The statistics in Table 1 are from the pertinent Bureau of Reclamation report (*7*, pp. 6–10). In the report these figures are not tabulated but appear in the text. When they are arranged as in Table 1 it is apparent, from the fact that columns 2 and 3 are identical, that they are not valid. Moreover, it is not known how the size of the drainage areas was determined to the second decimal point. Presumably this was done by acceptable methods. It appears, however, that studies of the sedimentation rate sufficiently detailed to justify this use of three significant figures were not made. It is obvious, when thus tabulated, that the sedimentation rate was stated to be 1.000 acre-foot per square mile per year for each of the five areas. This gives an erroneous and unjustified appearance of great accuracy. Even if there is an unpublished but acceptable basis for estimating a sedimentation rate of 1 (not 1.000) acre-foot per year per square mile for one of these areas, it would not necessarily be pertinent to the other areas. Their topography, vegetation, bedrock, and mantle are far from uniform. Instead of the claimed 80 years (*1*) required to fill the sediment storage space behind site *B*, it probably would be more nearly correct to say that the range of possibilities is from 8 to 800 years.

Table 1. Figures cited by the Bureau of Reclamation for the rate of sediment deposition in various parts of the Rainbow Bridge area.

Drainage area	Area (mi^2)	Sedimentation rate (acre-ft/yr)
Part of Bridge Canyon	6.54	6.54
Part of Bridge Canyon	0.84	0.84
Part of Bridge Canyon	7.62	7.62
Part of Bridge Canyon	1.05	1.05
Aztec and Bridge canyons	56.4	56.4

Some conservation leaders initially were impressed by the site *B* proposal before it was analyzed in detail. Now, however, conclusions similar to the above seem to be widespread, and as a result, the proposal for a restraining dam at site *C* is being supported instead.

Site C and Proposed Modification of Glen Canyon Dam

The situation that would prevail if a barrier dam were constructed at site *C*, three miles downstream from the monument, would differ markedly from the situation if one were built at site *B*. Because of the intervening distance, risk of collapse of Rainbow Bridge from blasting nearby, from undermining in the Kayenta formation, or from pseudokarst would be reduced to a minimum. At first it was feared that a diversion tunnel and barrier upstream from the monument might also be necessary, as they would be in the case of site *B*. However, a subsequent proposal to modify this plan, so that backwater at the base of the restraining dam

would be kept from rising to a level which would extend into Bridge Canyon, would obviate this. Such a plan would assure high-velocity flow of flash-flood waters past the narrows of Bridge Canyon, which otherwise would be an important barrier, causing rapid deposition of sediments in lower Bridge Canyon. This modification requires a larger pumping system than had been originally planned, and possibly intermittent dredging at the base of the site *C* dam, but no serious alterations in design or planning.

A dam at site *C* would not impair the beauty of the environs of Rainbow Bridge National Monument and would fully protect the monument itself. Access roads to the dam site could and should be built through the mouth of Aztec Canyon, and hence would be hidden by the rising waters of the reservoir. No scarring would be necessary away from the margins of the dam. Construction of an easy three-mile trail to Rainbow Bridge, above the level of the backwater, would involve no serious problems. Because Aztec Canyon has no important tributaries below site *C*, little silt would accumulate to block this arm of the reservoir, and access would be unimpeded. Otherwise, much silt would accumulate in lower Aztec Canyon whether or not a dam was constructed at site *B*. By obtaining material for the restraining dam from sources outside Aztec Canyon, ugly scarring of any adjacent area could be avoided.

A dam at site *C* would provide much greater protection in the event of flash-floods than one at site *B* (*7*). Moreover, it would permit eventual enlargement of Rainbow Bridge National Monument to include the portion of Bridge Canyon now downstream from the monument—an area which would thus be preserved in all its unspoiled magnificence.

It is not surprising that many conservation organizations have endorsed the site *C* project. On 18 January 1960 the executive director of the Sierra

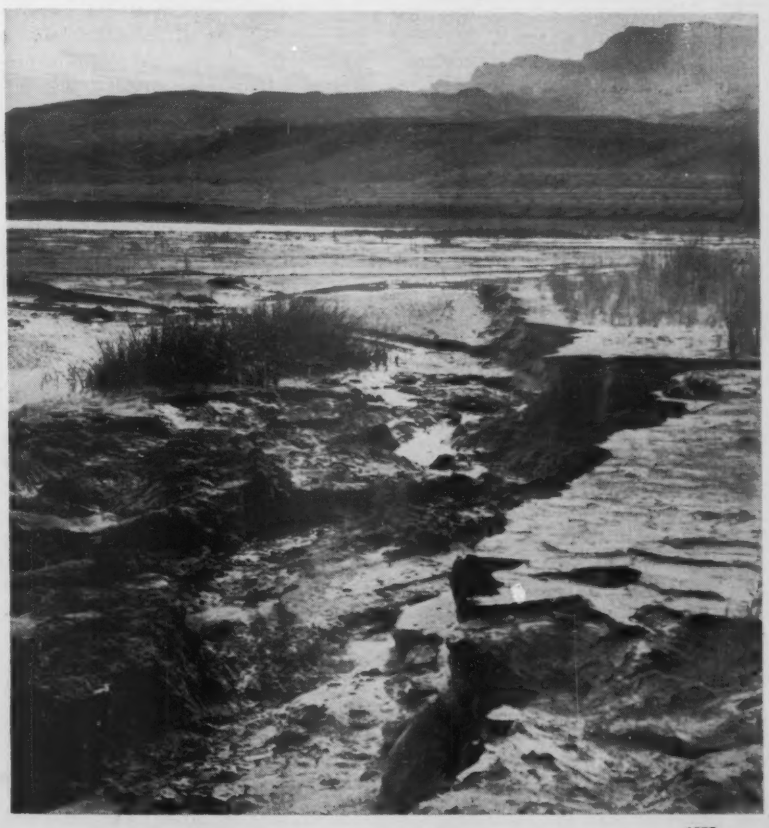


Fig. 3 (top). The zone of fluctuation of Lake Mead at a low reservoir level, showing aggradation. Such aggradation, it is claimed, would enhance Rainbow Bridge National Monument. [Sierra Club] Fig. 4 (bottom). The zone of fluctuation of Lake Mead at an intermediate reservoir level, showing aggradation of the kind anticipated for Rainbow Bridge National Monument if no protective project is constructed. [Harold C. Bradley]

Club (perhaps America's leading conservation organization) informed the Secretary of the Interior that site *C* was "far and away the best, and is a brilliant blend of engineering and scenic-resource planning" (*25*). This view seems entirely justified. There are, of course, problems associated with site *C*. Some are real. Others, upon analysis, appear artificial. As mentioned above, the backwater and silt accumulation upstream from the restraining dam should not be permitted to rise above 3450 feet, the elevation just below the Narrows, and thus a larger pumping system would be required than had been originally proposed. Transportation of equipment and materials to the site by barge would not be feasible since this would require postponement of construction until filling of the reservoir was under way. Unless provision were made for dropping the minimum level of the reservoir far below the present intake elevation, postponement of construction of this protective dam until barges could be used would not be safe, in view of the danger of sudden rises in the reservoir level, which are to be expected because of the sudden variations in flow of the Colorado River (*12*). The question of possible engineering difficulties at site *C* has been submitted to a consulting engineer, who has submitted the following report (here abridged) (*26*).

"A dam built at Aztec Canyon Damsite *C* would, if a pool is maintained above the dam, have water standing on both sides, and the base of the dam would be 140 feet below the dead water storage elevation of Glen Canyon Reservoir. There are engineering problems involved but they are not critical. If a masonry dam were built, there is a precedent in the Parker Dam built by the Bureau of Reclamation. The differential elevation of the water downstream and Havasu Reservoir is about 80 feet. If an earth-filled dam were built, the main difference would be that rip-rap would have to be provided on both sides. If a rock-filled dam, the rock-fill itself would provide the rip-rap. The material below the water on the low side would need to be designed for its net weight instead of its gross weight. The problem is one of cost rather than critical engineering considerations."

Contrary to Woodbury's statement (*1*), such a dam would not "require a fantastic investment." In 1955 the Bureau of Reclamation estimated that the cost would be $2 million to $4 million for any type of protective project (*15*). Two years later, the Upper Colorado River Commission estimated the cost at $3 million (*27*). Recently, the Bureau of Reclamation has increased its estimates to $15 million to $25 million for site *B*, and $25 million to $35 million for site *C* (*7*). Even these new figures are small in comparison with even the initial cost of Glen Canyon dam—$400-odd million exclusive of interest (*15*).

It is true that the site *C* dam would be an unnatural work, as Woodbury pointed out, but if Glen Canyon dam is completed, unnatural works of some kind inevitably will be present in the Rainbow Bridge area. The only unsettled matter is the question of what unnatural works will be permitted, and where they will be. Woodbury, in articles in 1954 and 1960, failed to recognize the major principle at stake: If there are to be destructive effects in the general area of a landmark that is important enough to require protection by law and solemn agreement, not only must the objectionable features be minimized but they must be outside rather than inside the boundaries of the area given that protection.

It therefore appears that a restraining dam at site *C* would provide a satisfactory and practical means of protecting Rainbow Bridge National Monument at reasonable cost, and with minimum risk to the stability of the great arch. However, this is not the only way to provide both a Glen Canyon reservoir and an inviolate Rainbow Bridge National Monument. The height of Glen Canyon dam might be lowered to about 3450 feet; this would permit high-velocity flow throughout Bridge Canyon and obviate the need for any protective project. This would, however, greatly reduce the power output and the cost of the dam, and the reclamation lobby, which has enormous political strength, would oppose it as bitterly as it would oppose abandonment of the dam project. Nevertheless, it would solve the problem of protecting the monument.

While it would appear reasonable to hold off the threats to the monument, to law, and to the integrity of Congress by merely halting temporarily the construction of Glen Canyon dam until the issue is resolved, this and similar proposals for a temporary halt have been effectively resisted by the monolithic reclamation lobby, even though it is probable that the site *C* dam site will be flooded by the reservoir before construction of a barrier dam can be begun (*11*). In 1960, under the influence of this lobby, Congress not only removed an appropriation for a protective project from the Public Works Appropriation Bill but included a provision which forbade the diversion of any other appropriation to the protection of Rainbow Bridge National Monument (*28*), despite the terms of the Colorado Storage Project Act and of the 1955 agreement. The chief objection to such a halt in construction is that it would delay the initial power output of the dam—a matter of great importance to reclamationists.

As a kind of compromise, certain hopeful conservation leaders urged that the reservoir be operated at a low level, below the elevation of site *C*, until the problem is resolved. This proposal is fiercely opposed by those who object to the delay in initial power production that would result, and, furthermore, it ignores the fact that, because of the curious design of Glen Canyon dam, the lowest intake of the dam and thus the minimum level of the reservoir are about 25 feet higher than the site *C* dam site. The same objection, and also that of headward aggradation into the national monument (*11*), applies to a similar proposal to hold the reservoir at an elevation of about 3590 feet. Because of this, the Sierra Club recently proposed modifying the design of the dam to include a lower intake, and hence a lower minimum level for the reservoir. This would provide a satisfactory temporary solution to the problem, even if a somewhat risky one because of the inherent human element. However, this suggestion has met with less than enthusiastic response from reclamationists.

Conclusions

From analysis of the available data it now appears that the Bureau of Reclamation either overstated its ability to protect Rainbow Bridge National Monument in 1954 (*7*) and 1955 (*15*), thereby obtaining Congressional approval of the Glen Canyon Project, or else is now exaggerating the difficulties (*29*). Law, specific agreement, and the conscience of the American people require that Rainbow Bridge National Monument be protected fully or that Glen Canyon dam be abandoned or greatly reduced in height. Contrary to the conclusion

recently expressed by Woodbury (*1*), flooding and aggradation in Rainbow Bridge National Monument would severely impair the monument and might threaten the stability of Rainbow Bridge.

Abandonment of the Glen Canyon dam project represents one extreme in this matter. Abandonment of Rainbow Bridge National Monument, or grudging provision for incomplete protection, is the other. As long as there is a satisfactory alternative, neither of these extremes is acceptable. It is evident that construction of a barrier dam at site *C* before construction is carried further at the Glen Canyon dam site represents a middle ground which should be acceptable to all but the extremists on both sides. Modification of the design of Glen Canyon dam to permit a lower minimum level of the reservoir might be worth while if infallible safeguards are set up, and if good faith is maintained better than it was in the case of the 1955 agreement. Obdurate reclamation leaders should beware lest the "default decision" they have sought endangers the entire Upper Colorado Storage Project. The American people do not like being double-crossed.

The new Secretary of the Interior faces a hard and immediate decision. He must choose between (i) constructing the restraining dam at site *C* essentially as outlined in this analysis; (ii) lowering the elevation of the top of Glen Canyon dam to 3450 feet; (iii) abandoning construction of Glen Canyon dam; (iv) lowering the minimum elevation of the reservoir, through redesign of Glen Canyon dam, to about 3300 feet, keeping it at that level until a dam is built at site *C*; or (v) violating law, solemn agreement, and basic conservation principles. Of these alternatives, only the site *C* plan, with or without lowering of the intakes of Glen Canyon dam, seems reasonable, and even this plan must be initiated speedily to avoid a disastrous outcome of this unfortunate and unnecessary situation.

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Reply by Woodbury

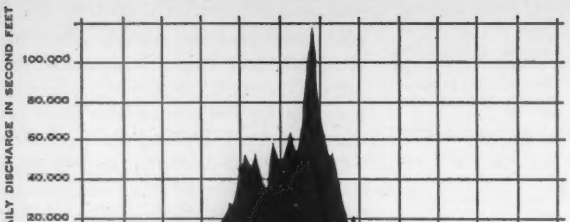
As Halliday has indicated in the preceding article, the protection of Rainbow Bridge National Monument presents a very complex problem. He complains that inadequate information has been accumulated in the past 5-year period to provide a sound basis for rational decisions, but he fails to note that 5 years ago I proposed (*1*) that Congress "authorize the development of the river basin, determine the policy of water use, provide funds for operation and refer minor items of dispute to some fact-finding scientific body for final adjudication as Sears (*2*) suggested. . . . If the Sears approach were accepted and the controversial matters were referred to scientific boards for investigation, then a positive approach for getting the unbiased facts for Congressional consideration and decision would be the main problem for scientists. The Congress would still have to set the policies and make the decisions, but it would have data carefully prepared by people trained in the art of fact finding."

Since the conservation leaders did not support my plea for factual studies, plans matured to "protect" Rainbow Bridge in accordance with the Act of Congress of 11 April 1956 (*3*), without any move being made to have the disputed problem studied by a fact-finding body of respected scientists. When I saw the plans developed by the Bureau of Reclamation and the National Park Service for protection of the bridge, I became convinced that they would do so much violence to the magnificent scenery in the adjacent area that the result would not be in keeping with the conservationist objective. This drove me to provide the factual information given in my article in *Science* (*4*).

Instead of accepting this as a contribution toward clarifying the complex problem, an anonymous editor (*5*) and Halliday (*6*) both attacked my article as partisan propaganda designed to confuse the issue. Halliday, in particular, quoted some statements of mine out of context and assigned distorted meaning to them. I refuse to be pushed into the role that has been assigned me by Halliday and the anonymous editor.

In none of my publications (*1*, *4*, *7*) have I recommended the construction of Echo Park reservoir. Instead of deciding in advance, as many other conservationists had done, that no reservoir should be built at that site, I called for a fact-finding study of the site, under the Sears proposal to supply Congress with a basis for rational, unprejudiced determination of the issue.

During my career I have been associated at different times with the U.S. Forest Service, the National Park Service, the U.S. Army, and the state of Utah and indirectly, through the University of Utah, with the Bureau of Reclamation. I am thus personally acquainted with many of the conserva-



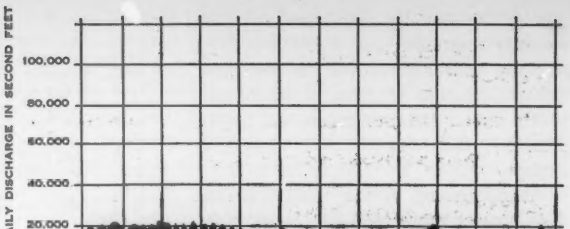


Fig. 1 (left). Hydrograph of unregulated stream flow at lower end of the Upper Colorado River Basin (at Lee Ferry) before the building of Glen Canyon dam. Fig. 2 (right). Hydrograph of regulated water flow in the Lower Colorado River Basin below Hoover Dam. [Upper Colorado River Commission]

tion problems of these and other agencies dealing with conservation of natural resources. With this background I can understand the need for conservation of forests, ranges, wildlife refuges, water supplies, natural areas, wildernesses, recreational areas, and other important resources as well as national parks and monuments.

For this reason I cannot take the extreme view advocated by many conservation leaders that establishment of an additional precedent of "invasion" of the National Park System must be avoided at all costs. It is my opinion that precedent-setting, important as it is, must be weighed against other values.

In my general article (*4*) I selected for emphasis those aspects of the problem that would give factual data regarding the plan proposed by the two government agencies concerned. In that plan I found no indication that site *C* was under serious consideration. I stated (*4*), "Two sites for such a dam have been studied, one in Bridge Canyon at site *B* . . . and another, much larger site, where construction would be much more expensive, in Forbidden Canyon below the mouth of Bridge Canyon (site *C* . . .). Site *B* has been selected by the Bureau of Reclamation and the National Park Service as most suitable. . . . Although construction of a barrier dam at alternate site *C* has been considered, it was excluded from the present proposal [of the two agencies]. From Fig. 8 it can be seen that this would be a much larger and more expensive dam than one at site *B*. It would require much larger pumping operations and would involve other complicating problems which need not be discussed here." Halliday's article now demands further discussion of these complicating problems.

Background

Many years ago, seven Colorado River Basin states made a compact to conserve the water of the river by impounding high waters, regulating stream flow, and dividing the water between the upper and lower parts of the basin. Congress made the decision to implement that agreement and provided for development of the lower basin first. Development of a suitable plan for the upper basin, comprising the heart of the great rough country of the American Southwest, presented more difficulties. It took a quarter century of intensive study of that colorful land of deep winding canyons, intricately dissected plateaus, and upthrust mountains to work out a practicable program to present to Congress.

The problem of devising a fitting program for water conservation was complicated by the potential value of the magnificent region for park, recreational, and other purposes. Many parts of the area might well be included in the national park, monument, and recreation system. Moreover, there are valuable mineral deposits (for example, uranium) in this rough country that provide further complications.

It was a decade before the Upper Basin Program, completed about 1946, was given partial approval by Congress. The delay was prolonged by the heel-dragging resistance of many conservationist groups, using political pressure to kill the Echo Park reservoir proposal and force inclusion of the amendment to the Glen Canyon reservoir proposal for the protection of Rainbow Bridge. At the same time, these groups failed to mobilize political support for preservation of other scenic areas of that colorful region, some of which are of even greater value, for National Park Service purposes, than Echo Park.

If scientists trained in specialized fields were authorized to make a fact-finding study of this remarkably varied and beautiful region, I believe they could provide, within the next 5 years, accurate data from which Congress could classify these lands according to their suitability for national parks, monuments, recreation areas, national forests, livestock ranges, wilderness areas, reservoirs, power sites, wildlife preserves, state parks, or other purposes, perhaps even including irrigation.

I envision a greatly expanded role for the National Park Service under such a classification program. Such areas as the triangle between the Green and Colorado rivers, Robber's Roost, San Raphael Swell, Goblin Valley, Cathedral Valley, the Circle Cliffs, Escalante River Canyon, Kaiparowits Plateau, the north foot of Navajo Mountain, the triangle between Colorado and San Juan rivers, the Needles, the Land of Standing Rocks, and other interesting areas might well be considered for inclusion in the park and monument system. Such a solution would give the government agencies involved a stable basis of action and help resolve many of the present conflicts.

The Upper Basin program provided for a dam in the main stem of the Colorado River in Glen Canyon, to store high water and regulate its flow out of the Upper Basin, much as the Hoover (Boulder) Dam had done for the Lower Basin, as illustrated in Figs. 1 and 2. As I understand it, the Bureau of Reclamation engineers designed the dam in Glen Canyon with a view to achieving maximum efficiency in the creation of a storage lake from which a regulated stream would run through the power plants to the Lower Basin.

This design calls for spilling water

at the 3700-foot contour level, considered by the Bureau to be the maximum operating level, above which no water can be stored. I am informed by the Bureau office in Salt Lake City that there is an 11-foot safety zone above this level, capable of containing the maximum probable flood, which greatly exceeds the largest flood of historical record. It is standard procedure to provide such safety zones in all dams. According to the most accurate forecasts obtainable with present techniques, there is little chance that any uncontrolled flood water would ever flow over the spillway after the initial testing had been completed. Flood waters above Glen Canyon will be stored, and streams will be regulated, by the Flaming Gorge Dam on Green River, the Curecanti dams on Gunnison River, the Navajo Dam on San Juan River, and other dams to be constructed. No overflow emergency is anticipated in Glen Canyon. No such uncontrolled overflow has occurred at the Hoover Dam in the Lower Basin, even though there have been no regulating reservoirs above it.

As an ecologist, I have a good working knowledge of the geology of this rough country of the Colorado Basin, having been associated with Herbert E. Gregory during my work as a naturalist in Zion Canyon and having been selected by him to lead a tour of members of the International Geological Congress through the region of Zion Canyon, the north rim of Grand Canyon, Bryce Canyon, and Cedar Breaks in 1933 (*8*). It was my conclusion, from my intimate knowledge of the geology and physiography of the Rainbow Bridge setting, that water from Lake Powell could not endanger the mammoth bridge.

Since Halliday has questioned my conclusion and has brought up hypothetical dangers of pseudokarst, water soaking, and enhanced erosion, I have consulted the eminent geologist Armand J. Eardly of the University of Utah, who has authorized me to quote him as saying that he sees no danger to the bridge from reservoir water standing under it. Figure 3 is a detailed view of the inner gorge from upstream, showing where the high-water mark would be, under the bridge, when the lake was at the 3700-foot maximum operating level. At a level of 3711 feet, water would still be confined to this inner gorge and would not approach the bases of the arch resting on the broad platform above.

There are many natural bridges, arches, and caves in the colorful Navajo sandstone cliffs of this region. The long life of these structures attests to the adequacy of the sandstone for supporting Rainbow Bridge for so long a time that the present discussions will be lost in antiquity before the majestic arch crumbles.

Alternatives

The real problem at issue is whether to (i) prevent the water of Lake Powell from backing up into the monument in accordance with the requirements of the existing law, or (ii) change the law to remove the requirements. Under (i) there are two further alternatives: to install the protective structures or to lower the level of Lake Powell. As for the first of these alternatives, it has been shown adequately (*4*) that installation of a barrier dam below the monu-

Fig. 3. The inner gorge under Rainbow Bridge, showing the prospective high-water mark with the reservoir at the maximum operating level of 3700 feet. [U.S. Bureau of Reclamation]

Fig. 4 (top). Soft, slushy mud left in the mouth of Bullfrog Creek after a flood in September 1957 [Stanley Rasmussen, U.S. Bureau of Reclamation] Fig. 5 (bottom). A small stream trickling through willows in an "inner gorge" in the mouth of Kane Creek. [Delbert Lindsay]

ment (site *B*) and a diversion dam and tunnel above it to divert water from Bridge Canyon would produce unacceptable marring of surrounding landscapes. A dam at site *C*, in Forbidden Canyon, would have the following disadvantages. (i) It would produce even more marring of the landscapes than a dam at site *B* because it would require about five times as much material, and the material would have to come from the high mesa adjacent to the monument, as shown in my earlier article (*4*, pp. 520, 523). Of the other scars at the dam site, those near the bottom would be permanently covered with water, those at the top would be permanently exposed, and those between would be periodically covered and uncovered by the fluctuating water level. (ii) Much more water and sediment would collect at site *C* than at site *B*, and larger pumping operations would be required. (iii) A dam at site *C* would constitute a barrier to navigation in the lake; visitors to the bridge would have to dock their boats on one side, climb over the dam, and obtain different transportation on the other side. (iv) Installation and maintenance of a dam at site *C* would require heavy financial investment. (v) The dam would provide only temporary protection for the monument because the unwanted lake above the dam would eventually fill with sediment and back up into the monument in a delayed "invasion" from man-made works. And (vi), it would be impossible to build the dam at the present time without disrupting the development of the Glen Canyon project. Halliday's "easy three-mile trail" from site *C* to the bridge is much less feasible to construct than he indicates. The alternatives would be to cut the trail in the face of the cliffs above the water level or to construct a very difficult trail along the top of the deeply eroded ridge beside the canyon.

As for the second alternative, lowering the level of Lake Powell from a maximum operating level of 3700 feet to a level of approximately 3600 feet would have the following disadvantages. (i) It would reduce the storage capacity of the lake from approximately 28 million to less than 15

million acre-feet of water—a loss of nearly half the storage capacity and of much more than half the usable storage space above the minimum dead-level contour of 3490 feet. The water that thus could not be stored at this reservoir site would have to be stored in some other reservoir in the Upper Basin where evaporation would be greater than from Lake Powell, unless it could be stored in Echo Park, use of which as a reservoir is not authorized. (ii) Lowering the water level would result in a tremendous reduction in the amount of electric power that could be produced at Glen Canyon. (iii) Drastic revisions of the design of the Glen Canyon dam and power plants would be required. (iv) Dam construction would be interrupted. And (v), the construction contracts would have to be revised.

As for changing the law to remove the existing requirements, if Congress should take a new look at past commitments and decide that the cost of protecting Rainbow Bridge would be unjustifiably high, it could certainly change its policy and correct past mistakes. If it decides to revise the law for this purpose, then development of the Glen Canyon project could proceed on schedule to provide storage of high water and regulation of stream flow.

In that event, the sliver of water from Lake Powell would back up under the bridge and rise and fall in accordance with the fluctuations of the lake. Both high water and erratic floods from Navajo Mountain would deposit sediment and debris at the end of the sliver of water at whatever level it happened to occupy at the time. Since much of the sediment in Bridge Canyon would be sand, the deposit at the mouth of the stream would probably be much less "gooey" than that shown in Fig. 4. When finally filled and overgrown with vegetation, the inner gorge would probably look something like the partly filled gorge shown in Fig. 5. Both of these pictures are views from Glen Canyon.

Discussion

The essential question of debate here is one of precedent. If invasion of the monument is authorized, the whole question will be automatically settled and nothing further need be done about it. The price paid, if this is the decision taken, will be the sliver of water under the bridge, filling of the bottom of the gorge with sediment, and eventual covering of the sediment with vegetation. There is nothing that I can discern in the geology and physiography of this deeply eroded region, where geology is an open book, to substantiate Halliday's far-fetched contention that water standing in the inner gorge would constitute a significant threat to the foundations of Rainbow Bridge. During the period when the gorge was filling with sediment, there would be a lot of messy deposits of debris and sediment in Bridge Canyon. During this interim period, measures could be taken to give visitors access to the bridge through Bridge Canyon. The administrative costs of such measures would be about equal to costs of similar procedures at a lake above a barrier dam.

The price to be paid if invasion of the monument is not authorized would be the cost of installing and maintaining protective works and the marring of surrounding landscapes or interference with the Glen Canyon project. Refusal to give such authorization because a precedent is at stake would hardly seem justified in view of the fantastic sum that such a refusal would cost society.

There is another consideration, one not discussed by Halliday. Any reservoir above any barrier dam that might be placed in Bridge or Aztec Canyon would eventually fill with sediment and back up into the monument. This means, then, that any protective barrier dam could only serve as a temporary expedient and could not permanently prevent the invasion of the monument by debris and sediments. Such installations would only delay the invasion.

Furthermore, Bridge and Aztec canyons are both part of the magnificent areas surrounding the bridge, and messy deposits of silt and debris above a barrier dam in either of these canyons would be just as devastating as silt and debris inside the monument. The final argument, then, simmers down to a question of whether the gorge under the bridge should be filled first or last, now or later. To fill it now would cost practically nothing. The alternative plan would require a fantastically large investment, and would not permanently achieve its objective; eventually the gorge would be filled.

This is a case that calls for soul-searching on the part of conservationists. To make a decision first and then hunt for evidence to support that position is not the method of science. Taking an adamant stand that ignores opposing evidence will not inspire the degree of confidence that is to be derived from open-minded evaluation of all evidence. If conservationists would take this objective attitude and expend their energy in studying the problems of this last frontier of the rough country of the Southwest, they could build much more strength into the National Park Service and the conservationist movement than they do by dissipating their efforts and arguing from unrealistic assumptions. Can it be that some conservationists are operating in such a deep groove of dogma that they cannot see the plain facts staring them in the face?

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Clyde Kluckhohn, Anthropologist

Clyde Kluckhohn died suddenly of coronary thrombosis on 28 July 1960 in Santa Fe, New Mexico, at the age of 55. His friends had known that he had suffered from a serious heart condition for many years, but subject to that there had been nothing unusual about his state of health until he underwent a severe attack the day before his death.

Kluckhohn was not only one of the few leading American anthropologists of his generation, but also one of those who did most to establish the present level of close interdisciplinary relationships with its academic neighbors, among which sociology and psychology figure most prominently. This aspect of his many-sided career and influence, which is naturally of particular interest to sociologists, was perhaps most conspicuously evidenced in his role as one of the principal planners and founders of the Department and Laboratory of Social Relations at Harvard University, and as the leading senior anthropologist in that department since its foundation in 1946. He was also highly instrumental in promoting and organizing interdisciplinary work—involving a number of sociologists, psychologists, and others—through his directorship of the Russian Research Center at Harvard for its first five years, from 1948 to 1953. Among a variety of other research enterprises, he had a paramount role in the comparative study of values in five cultures, which was carried out in New Mexico from 1949 to 1954.

Kluckhohn was born in Le Mars, Iowa, in 1905. He prepared for college at Lawrenceville Academy, New Jersey, and began his undergraduate studies at Princeton. These were, however, interrupted by ill health and, only after an interval, concluded at the University of Wisconsin. This was, however, one of the "ill winds" of legend, since Kluckhohn went to New Mexico for his health and in this connection developed his first interest in anthropology through the presence of the Navaho in the immediate neighborhood of his sojourn.

After graduation from Wisconsin he went to Oxford as a Rhodes Scholar, where in particular he came in contact with R. R. Marrett. Then he spent a year at the University of Vienna, studying both anthropology and psychoanalysis. After a brief teaching interval at the University of New Mexico he came to Harvard to complete his graduate work. After he received his Ph.D. in 1936, his whole formal academic career was, with various leaves of absence, spent at Harvard.

The cosmopolitan catholicity of Kluckhohn's choice of places of education was matched by that of his substantive interests. His undergraduate concentration was in classics, with special reference to Greek. This, combined with the linguistic problems of anthropological work, led him into the field of linguistics, which was one of his main lifelong scientific interests. He became thoroughly schooled in physical anthropology and its foundations in general biology and was an important contributor here. He was one of the very first social scientists to undertake a serious study of psychoanalytic theory and its possible relevance to anthropology, an undertaking which included a personal analysis in Vienna. This interest paid off in a number of his anthropological studies, but particularly perhaps in his notable monograph, *Navaho Witchcraft*.

The main trend of Kluckhohn's intellectual interests, however, came increasingly to focus on the problems of culture in relation to human behavior and, within this field, in particular the study of values. For a considerable period he was accounted one of the leading members of the "culture and personality" school of thought, but it can be said that he transcended this framework in his later years, in an altogether original approach to the study of values. This crystallized in connection with the comparative study referred to, and was much further developed in an important paper on American values, and one or two recent theoretical papers.

With this increasingly definite focus on values, however, it can certainly be said that Kluckhohn was, for his generation, par excellence the *general* anthropologist, a role which was expressed on the more popular level by his *Mirror for Man*. This role as generalist for anthropology was intimately connected with his concern for, and receptivity to, the interests of his neighboring disciplines, which was such a conspicuous feature of his career.

It was not only intellectually, in the more strictly academic sense, that Kluckhohn was a man of many and diverse interests and talents. He was an academic administrator of the highest capacity, as evidenced among a number of cases by his most unusual contribution as director of the Russian Research Center. He was a man of the most balanced and penetrating judgment and was much in demand in this connection, above all in the field of public affairs where he was a consultant in many different connections and on the highest levels but also very much on university and foundation matters. Furthermore, he was not only a student of his beloved Navaho, but was also one of their most powerful friends in their complicated relations with the federal government, a cause to which he devoted untold exertions. That he was also a man of extraordinary personal magnetism and charm, all who have been privileged to know him can testify.

Kluckhohn made major contributions to sociology, from the vantage-point of his deep commitment to his own discipline of anthropology, which were brought to focus, in particular, in the intellectual partnership, which was one important aspect of his marriage, with Florence Rockwood Kluckhohn. Mrs. Kluckhohn is a distinguished sociologist in her own right, and for a number of years has been a leading member of the sociological group in the Department of Social Relations. This marriage both symbolized and promoted the fundamental unity of the behavioral sciences of which Clyde Kluckhohn's career is one of the major monuments. His death, combined with that of his close friend and colleague, Samuel A. Stouffer, which followed it within less than a month, creates a void in this field which can never be filled, not only at Harvard, but in the national and international fields of social science.

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Note

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Science in the News

Missiles vs. Bombers: Congressional Committees Express Some Doubt

The Congressional Armed Services committees, both of which issued their reports on aircraft and missile authorizations last week, went pretty much down the line with Kennedy's defense budget, with the single significant exception of manned bombers. Neither the Eisenhower budget, nor Kennedy's revisions, asked for any new money to buy bombers. Defense Secretary McNamara told the committees that no firm decision had been made to buy *only* missiles in the future. He said that, under orders already placed, bomber production would continue into the fall of 1962, and that therefore a definite decision about shutting down the factories could be postponed until next year.

Both House and Senate committees, nevertheless, authorized several hundred million extra dollars, earmarked for bombers that the Defense Department, so far, has no intention of buying. Both committees offered the same three arguments for not shifting too quickly to sole reliance on missiles, two of which were curious.

The basic argument for bombers in the age of missiles has centered on the greater flexibility of manned aircraft: it can seek out targets, it can bring back reconnaissance reports, it can be recalled, and so forth. The basic argument against them has been that within a few years it will be exceedingly difficult for a bomber to fly over enemy territory without being shot down. The useful life of the planes can be extended beyond this point by the development of long-range missiles that can be carried by the planes and which will make it unnecessary for the bombers to fly over enemy territory. But this eliminates some of the major advantages of the manned plane, since it no longer could seek out targets or bring back damage reports. The airborne ballistic missiles then, are not being developed because they are so valuable in themselves, but mainly because they will prolong the usefulness of the expensive fleet of existing bombers. It is doubtful that, once bombers have lost their usefulness as bombers, the Defense Department would buy any more merely to serve as airborne missile launchers.

Such considerations seriously weaken the basic argument for manned bombers, and perhaps because of this, the Armed Services committees emphasized two new arguments in favor of more bombers. "Is it not entirely possible, indeed hopeful," said the House committee, "that nuclear weapons might by international agreement be outlawed at some time in the future? Would not at that time the nation who possesses a conventional capability be in a virtually absolute position with respect to his enemies? Also—and this is a thought which to the knowledge of the committee has not been stressed in the past—who knows whether an intercontinental ballistic missile with a nuclear warhead will actually work? Each of the constituent elements has been tested, it is true. Each of them, however, has not been tested under circumstances which would be attendant upon the firing of such a missile in anger.

"By this the committee means an intercontinental ballistic missile will carry its nuclear warhead to great heights, subjecting it to intense cold. It then will arch down and upon reentering the earth's atmosphere subject the nuclear warhead to intense heat. Who knows what will happen to the many delicate mechanisms involved in the nuclear warhead as it is subjected to these two extremes of temperature?

"The scientists may say that all of these things are determinable by extrapolation. Perhaps this is so. To the committee, however, it seems that our only knowledge of the actual workability of an ICBM fired in anger is in textbooks and in laboratories. The committee is unwilling to place the safety of this country in a purely academic attitude, and for this reason has added to the bill authorization for bombers."

The Senate committee, less emphatically, made substantially the same points. Defense officials were generally unimpressed by either argument. What if a non-nuclear general war were to develop after it had become highly improbable that a significant part of a bomber fleet could get through enemy defenses; how useful would bombers carrying conventional weapons be, where to cause any significant damage large numbers of bombers would have to reach their targets, survive the flight, and return to reach their targets again? Or, more to the point, how useful, considering the likelihood of such a situation arising, would an extra 50 bombers be compared to something else that might be bought for half a billion dollars or so?

The Defense Department officials were even less impressed by the argument questioning whether we could rely on missiles merely because the scientists assured the country they would work.

They pointed out that the detonating mechanism had been repeatedly tested in actual launchings, with inert material substituted for fissionable material, and that it was strange that at this late date anyone should wonder whether an atomic weapon really would go off once the detonating mechanism had worked properly. They suggested that since Commander Shephard had survived the "intense cold" above the atmosphere and the "intense heat" of the re-entry without being either frozen or fried, it was strange that anyone should seriously question the ability of a mechanical device to do as well. Indeed the whole argument was strange, including the final warning about the House committee's unwillingness "to place the safety of this country in a purely academic attitude."

Air Force Position

The report was a paraphrase of the points that the Air Force witnesses had made in questioning the tendency of the Defense Department to write off manned bombers. The cancellation of the atomic airplane project and the sharp cutback on the development of B-70 bombers were other reflections of this tendency.

But neither the Air Force generals

nor the committee members really attach much importance to the argument stressed in the report questioning whether the scientists' "academic" judgments were to be trusted. What has happened is that the manned-bomber supporters, having arrived at an intuitive feeling that we may be writing off the bombers prematurely, have offered any explicit arguments that came to mind to support this conviction.

Both committees arrived at their decision unanimously, something which was comparatively easy to do under the circumstances, since a decision to authorize the money is a long way from a decision to spend the money. The Appropriations committees must first appropriate the money authorized; then the Administration must decide to spend the money. Congress' only recourse if the Administration refuses to buy bombers despite an appropriation is to impeach the President for misconduct, which is merely a theoretical possibility. As a practical matter the Administration does not have to spend the money, and the effect of the budget change is to remind the Administration that Congress is sufficiently impressed with the case for bombers to make money available in advance, in case the Administration should have a change of heart.

Committee View

The views of the individual committee members ranged from a strong conviction that the money ought to be spent to a feeling that there was no real harm done in making the money available. As a result neither report urged the President to spend the money. The House report came close to implying this, but the Senate report merely spoke of giving the Administration the "option" to spend the money.

On another phase of the bomber-vs.-missile debate the division within the committees was sufficiently sharp so that both committees avoided the question. The Administration had ordered the research and development effort on the 2000 mile an hour B-70 bomber cut in half.

This would mean about a year's delay (until 1969) in case the Defense Department decided it wanted the bomber, but would save $1.5 billion if, as seems more likely, the plane is not wanted after all. (It was the Democrats, including Kennedy, who pressed Eisenhower to raise spending on the project; Kennedy has now cut Eisenhower's budget recommendation, and Barry Goldwater has become the most outspoken advocate of the plane.)

Until this year the authorization bills for the armed services have been written in very general language. ("The Secretary of the Air Force may procure and construct guided missiles.") Last year, to give the Armed Services committees more authority to review what the Defense Department is doing, a law was passed requiring more specific authorizations for aircraft, missiles, and ships. The Air Force chose to interpret this very broadly and submitted its budget for research and development of aircraft and missiles, in addition to actual procurement. This enabled the Air Force to put its case for the B-70 bomber before the committee. Both the civilian secretary of the Air Force and the chief of staff opposed the decision to cut back work on the bomber, but both committees declined the opportunity to add a specific authorization for more work on the B-70. On the other hand, both committees made it clear that they had chosen to consider the matter outside the scope of authorization required under the new law; this left the Appropriations committees still free to add extra money for the B-70 anyway.

The controversy over the bombers extends into the Air Force itself. The Air Force's civilian operations analysts at the Rand Corporation have shown little sympathy for the bombers, and there is a faction in the Air Force which agrees with the easily encountered view of Air Force critics that the predilection for bombers is in good part a psychological quirk.

Barring the development of manned weapons in space, which appears to be a long way off, the Air Force, in regard to its major mission, the maintenance of the strategic deterrent force, will be grounded within a few years. Its greatest responsibility, after the bombers are gone, will be to sit on the ground waiting to push buttons which everyone hopes will never be pushed. Of course, everyone hopes the bombers will never be used to bomb anything. But the routine of keeping ready when you have bombers involves a good deal of flying around, which comes closer to the Air Force's idea of what an air force ought to be doing than sitting in a hole in the ground keeping a close eye on the buttons.

On the other hand, there is no indication that the Air Force has let its concern about becoming a "static service" seriously interfere with the business at hand. General White, the Air Force chief of staff, was asked whether he felt strongly enough about the bombers to cut out some missiles to make room for them within the recommended budget. He promptly answered that the Air Force would like some extra money for bombers, not a substitution of bombers for missiles. To a good many Air Force officers concern over becoming a static service has been allayed by the decision, over the opposition of the Army and Navy, to give the Air Force responsibility for future Defense Department space projects.

Oil Pollution

The Senate last week was asked to ratify a treaty signed 7 years ago to control the pollution of the sea by freighters and tankers dumping oil. The 7 years seem to have been taken up while the State Department carried on leisurely negotiation within the country to win the approval of any group that might oppose the treaty. Since, even with the delay, the United States is the 13th out of 42 signers to ratify, and since this type of pollution in American waters is already controlled by laws stricter than the treaty, the Foreign Relations committee expressed curiosity but not annoyance at the delay. But the State Department had been efficient in quieting whatever opposition there may have been. The treaty was ratified 92 to 0.

Summit Talk

The first hint of Kennedy meeting with Khrushchev appeared in a column by James Reston in the New York *Times*. Reston reported that despite the heavy pressure to concentrate his attention on current crises, such as Cuba and Laos, the President had not forgotten the more important issues.

"The great turning point of history now," Reston wrote, "is not Cuba or Laos, important and troubling as they both are, but the control of nuclear armament and the movement toward unity in this hemisphere, in the Atlantic, and in the free world."

The inevitable pressure to hand the Russians an ultimatum to come to terms on the test ban has developed. Senator Dirksen, the minority leader, called for such an ultimatum last week, and a resumption of underground testing if it is not fulfilled.

Kennedy, Reston reported, was rejecting this advice. "It can be said with some confidence that he is determined to have a personal talk with Premier Khrushchev before he reaches so crucial a decision."

Announcements

American Academy Elections

At its 181st annual meeting in Boston, 10 May, the American Academy of Arts and Sciences elected **Hudson Hoagland** as its 31st president. Hoagland, who is executive director of the Worcester Foundation for Experimental Biology, succeeds **Kirtley F. Mather**, professor emeritus of geology at Harvard. The following vice presidents were also elected:

William P. Allis, professor of physics, M.I.T. (mathematical and physical sciences).

Frank M. Carpenter, professor of zoology, Harvard (biological sciences).

Francis Keppel, dean of the Faculty of Education, Harvard (social arts and sciences).

J. P. Elder, dean of the Graduate School of Arts and Sciences, Harvard (humanities).

J. L. Oncley, professor of biological chemistry, Harvard Medical School, was elected secretary.

In addition, 100 new fellows were elected, as well as 34 foreign honorary members from various countries, including Argentina, Australia, Canada, England, France, the Netherlands, Scotland, Spain, Sweden, Switzerland, Israel, Japan, Turkey, and the U.S.S.R.

Courses

A course on **scientific and technical abstracting and indexing** will be offered by Columbia University's School of Library Service during the summer session, 3 July to 11 August. The course will cover various uses to which scientific and technical abstracts are put, types of abstracts suitable for particular uses, techniques of indexing abstracts, and instruction and practice in abstracting. Requirements for admission are a bachelor's degree, some professional experience, and adequate scientific background. (School of Library Service, Columbia University, New York 27)

The Atomic Energy Commission is accepting applications for its 1-year course in **radiation physics and biophysics**, to train college graduates for positions in state, county, or municipal departments that have responsibility for public health and safety in matters dealing with radiation. Beginning in the fall term of 1961–62, trainees will spend an academic year at the University of Michigan or at Harvard obtaining basic knowledge in this field. They will then go to an AEC installation for 8 to 10 weeks of practical training. There they will work on health-physics problems of monitoring, dosimetry, instrument calibration, shielding, and waste disposal. Employees or potential employees of governmental departments which have responsibility for radiation control are eligible to apply for the course. An applicant should have a bachelor's degree and an adequate background in science (preferably including mathematics through calculus). However, majors in public health who do not fully meet these academic criteria will be considered. Completed applications, letters of reference, and academic transcripts should be submitted *by 15 June*. (Oak Ridge Institute of Nuclear Studies, Oak Ridge, Tenn.)

Grants, Fellowships, and Awards

The American Academy of Arts and Sciences is offering three prizes of $1000, to be awarded annually to the authors of **unpublished monographs**—one each in the fields of the humanities, the social sciences, and the physical and biological sciences. A monograph is defined as a "scholarly contribution to knowledge, too long for an article . . . and too specialized for a general book." The final date for receipt of manuscripts is *2 October*; announcement of the awards will be made in December. (Committee on Monograph Prizes, American Academy of Arts and Sciences, Little Hall 33, Harvard University, Cambridge 38, Mass.)

Funds are available for a limited number of travel stipends to the 5th Pan American Congress of **Endocrinology** to be held in Lima, Peru, 15–21 October. The stipends are for younger investigators wishing to present papers at the congress. Applications, accompanied by an abstract of the paper to be presented, should be received by *15 July*. (Gregory Pincus, Worcester Foundation for Experimental Biology, Shrewsbury, Mass.)

Summer research fellowships of $500, offered by the Tobacco Industry Research Committee, are available to students in the nation's 90 accredited medical colleges. The program, now in its 7th year, is designed "to encourage students to make a career in research." Each student fellow is selected by the dean of his medical school and may conduct research in any field of his choice. One student from each medical school may receive a fellowship. Applicants must be qualified to undertake summer or other off-term study under the supervision of experienced scientists.

Scientists in the News

The American Psychiatric Association has awarded the 1961 Hofheimer prize to a team of scientists for studies, carried out at the Columbus Psychiatric Institute of Ohio State University, on the effects of stress on animals. The results of the research indicated that animals which were subjected to stress during the first 2 weeks of life were later sturdier, more emotionally stable, and performed better on learning tasks. The studies were made under the direction of **Benjamin Pasamanick**, professor of psychiatry. Members of the team of scientists are **Seymour Levine**, psychologist; **Morton Alpert**, anatomist; **Carl Cohen**, microbiologist; and **George Lewis**, medical student at Ohio State.

Hampton L. Carson, professor of zoology at Washington University, has been elected a Fulbright research scholar to study population genetics at the University of Melbourne, Australia.

Arthur H. Wolff, assistant chief of the research branch, Division of Radiological Health, U.S. Public Health Service, has been assigned to serve with the Atomic Energy Branch of the U.N. Food and Agriculture Organization, in Rome, as a special consultant on radioactive materials in food and agriculture. He will set up training programs on radioactivity and food hygiene in coordination with the World Health Organization and the International Atomic Energy Authority.

Five health physicists have been named recipients of Atomic Energy Commission special fellowships for advanced training, administered by the Oak Ridge Institute of Nuclear Studies. The fellowships are for 1 year of advanced study leading to the Ph.D. degree in disciplines closely related to health physics. The recipients are **James C. Couchman**, Convair, Fort Worth;

Richard L. Lehman, Lawrence Radiation Laboratory, University of California; **Vaughn C. Moore**, University of Minnesota Hospitals; **Richard D. Neff**, Tison-Pease Co., Los Angeles; and **Donald Willhoit**, University of Pittsburgh.

Leslie B. Arey, anatomist and professor emeritus at Northwestern University Medical School, has been named an adviser to the Laboratory of Perinatal Physiology, San Juan, Puerto Rico. The laboratory is part of the National Institutes of Health.

Leona Baumgartner, associate professor of pediatrics at Cornell Medical College and commissioner of health, New York City, and **Roger Revelle**, dean of the University of California's School of Science and Engineering and director of the Scripps Institution of Oceanography, have been named members of the Advisory Council for the Peace Corps.

Howard J. Teas has been appointed head of the recently created Agricultural Bio-Sciences Division of the Puerto Rico Nuclear Center, University of Puerto Rico, Mayaguez. He was previously associate professor of botany at the University of Florida Agricultural Experiment Station.

B. P. Pal, director of the Indian Agricultural Research Institute, New Delhi, has been awarded the first Rafi Ahmed Kidwai memorial prize for agricultural research. He received the award for his work in breeding rust-resistant varieties of wheat.

Stephen J. Angello, project manager of Westinghouse Electric Corporation's new products laboratories, has been named visiting Mackay professor of electrical engineering at the University of California, Berkeley.

Robert M. Hexter, senior fellow of the Mellon Institute, Pittsburgh, has received a Guggenheim fellowship and a Fulbright grant to engage in research on the electronic and vibrational spectra of molecular crystals at the Israel Institute of Technology, Haifa.

John S. Boyce, Jr., forest pathologist with the Southeastern Forest Experiment Station, Asheville, N.C., will join the University of Georgia's department of plant pathology and plant breeding on 1 July.

Four Westinghouse scientists have been named to participate in the newly established Westinghouse academic leave program, which permits selected laboratory personnel to carry out individually planned research and study at a university or nonprofit institution of their choice anywhere in the world.

B. S. Chandrasekhar, physicist, will become a visiting scientist at Imperial College, London.

Kan Chen, engineer, will conduct research in systems engineering at Stanford.

Donald P. Gaver, mathematician, will teach and carry on research in probability theory and statistics at Stanford.

Stanley L. Ruby, nuclear physicist, will hold a research fellowship at the Israel Atomic Energy Commission's research reactor establishment and at the Weizmann Institute, Rehovoth.

Irving H. Siegel, formerly a member of the senior staff of the President's Council of Economic Advisers, has been appointed chief of the new Military Economics and Costing Division of Johns Hopkins University's Operations Research Office.

Sam E. Stephenson, assistant professor of surgery, and **Grant W. Liddle**, associate professor of medicine, at Vanderbilt University, have been appointed associate directors of the university's Clinical Research Center.

Oswald K. Sagen, formerly chief of special studies in the U.S. National Health Survey, has been appointed chief of the National Office of Vital Statistics and assistant director of the National Center for Health Statistics, U.S. Public Health Service.

Edward A. Adelberg, chairman of the University of California's department of bacteriology, has been appointed professor of microbiology and chairman of the department at the Yale University School of Medicine.

Lawrence Z. Freedman, fellow at Stanford University's Center for Advanced Study in the Behavioral Sciences, has been named to a new professorship at the University of Chicago —that of research professor of psychiatry. He will head an intensive psychiatric study of nonconformist behavior. The project was established with a grant from the Foundations' Fund for Research in Psychiatry.

Thelma C. Heatwole, patent liaison officer for the Philip Morris Company, has been honored by the Virginia Academy of Science for her leadership of the Junior Academy of Science over a 10-year period.

Georg von Bekesy, member of the faculty of Harvard University, has received the gold medal of the Acoustical Society of America for his research on the function of the human ear.

Peter G. Meek, administrator with the National Society for Crippled Children and Adults, has been appointed executive director of the National Health Council.

Yoshio Tanaka, research associate with the Geophysics Research Directorate, Air Force Cambridge Research Laboratories, Bedford, Mass., has been selected as the Guenter Loeser memorial lecturer for 1961. He was selected in recognition of his contributions to molecular spectroscopy, especially to knowledge of the structure of atmospheric gas molecules.

John S. Toll, chairman of the University of Maryland's physics department, has been elected chairman of the Federation of American Scientists. Other officers elected are **Peter G. Bergmann** (professor of physics, Syracuse), vice-chairman; **William C. Davidon** (Argonne National Laboratories), secretary; and **M. Stanley Livingston** (M.I.T.), treasurer.

Recent Deaths

Charles F. Crampton, 32; assistant professor of pathology and biochemistry, College of Medicine, University of Florida; formerly associated with the Rockefeller Institute; 23 Apr.

Hugo Roesler, Philadelphia; 62; cardiologist and associate professor of medicine at the Temple University Medical Center, Philadelphia; specialized in x-ray studies of the cardiovascular system and in electrocardiography; 26 Apr.

Frederick R. Wulsin, 68; anthropologist and member of the Secretary of the Army's advisory panel; 26 Feb.

Erratum. In the article "Behavioral thermoregulation," by B. Weiss and V. G. Laties [*Science* **133**, 1338 (28 Apr. 1961)], the third sentence of the legend to Fig. 5 (p. 1341) should have read "Rats 2–1, 4–NN, and 0–1 were also run at an intensity setting of 75 watts." In the published article, rat 3–1 was erroneously included in the list given in this sentence.

Book Reviews

Principles of Animal Taxonomy. George Gaylord Simpson. Columbia University Press, New York, 1961. xii + 247 pp. $6.

The deepest foundations of taxonomy and classification are examined in this book based upon the Jesup Lectures given in 1960 at Columbia University. The illustrative data are selected from zoology, particularly mammalian zoology, but the principles also apply to botany and microbiology. Taxonomy is considered to be the theoretical study of classification, including its bases, principles, procedures, and rules, while systematics is viewed as the scientific study of the kinds and diversity of organisms and of any and all relations among them. Simpson stresses the reality of the relations of individuals in populations and the relations of populations constituting taxons.

The author emphasizes the interdependence of taxonomy and evolution and refutes the attitude that taxonomy is self-sufficient and sharply distinct from other biological sciences. The whole organism including all of its parts and aspects, its physiology, embryology, behavior, ecology, and biogeography must be taken into account by the taxonomist.

All types of characters do not have equivalent value for taxonomic or phylogenetic interpretation, and various criteria for the comparison of characters are discussed. Simpson quite correctly states that evolutionary classification involves both neontology and paleontology, and he shows the values of each separately and together. Vertical evolutionary time and horizontal relationships, both in the past and present, are integrated.

Although he might have made a better case for the best nomenclatural system to be found in any of the biological sciences, Simpson properly does not exaggerate the importance of nomenclature in taxonomy. The monotypic higher category is justified on sound grounds of relationships and logic. He discusses many theoretical points that can serve as useful deductive guides in making taxonomic decisions. He has not written an *apologia* in order to build up a defense against the shortsighted and sometimes stupid attacks from narrow specialists in other disciplines. He emphasizes the value of other sciences to taxonomy more than the value taxonomy may have for a multidisciplinary approach to the life sciences. However, I feel sure that he is keenly aware of the role of taxonomic research and interpretation in the advancement of other biological sciences. This book is definitely a contribution to the philosophy of science rather than a handbook for the taxonomic specialist.

Controversial Questions: Alternative Views

There is far more to praise than to criticize in this volume, but it behooves a reviewer to raise some controversial questions and to present some alternative views for consideration.

I agree with Simpson that taxonomy has eminent esthetic value. It is devoted to ordering complex objective data, and harmonious order lies at the root of esthetic appreciation. However, in my opinion, Simpson exaggerates the art of the taxonomist. The taxonomist, along with all other scientists, has to use tentative working hypotheses when the evidence is insufficient for clean-cut interpretation. A working hypothesis, even though ultimately shown to be incorrect, should not be assigned to arbitrariness, subjective bias, or artistry, with the implication that it is divorced from facts and scientific logic. Simpson says that the concept of a taxon is invariably subjective. It would be better to call a taxon an abstraction founded upon objective sensory experience. As in other sciences, imagination is necessary in taxonomy and complete information is never attained. Epistomological method and theory apply equally to all sciences.

Simpson examines the concept of the species with much critical penetration and includes the evolutionary time dimension of the entity. However, in my opinion, he could have incorporated the various attributes of the species within a much more adequate operational definition than that of Ernst Mayr's, which he chose to discuss.

His inquiry into the designation of type specimens is, to my mind, inadequate in both theory and practice. The modern taxonomist does not have to adhere to the typological concepts of an earlier age. Greater accuracy of nomenclature, of identification, and of handling growing knowledge can be attained by the designation of type specimens that include several categories of secondary types, which is a practice Simpson finds unnecessary, confusing, or ridiculous.

Although I do not disagree with his definition of homology as "resemblance due to inheritance from a common ancestry," he might have emphasized the genetic component more. In the past Simpson has contributed immensely to a synthesis between genetics and paleontology. In this book he does not think it practical or theoretically desirable to base homology on identity of gene components. He says: "If a given characteristic is continuously present in an ancestor and in all the descendents of a given lineage, then it is homologous throughout even though the genetic substrate has changed." "Genetic evolution and somatic evolution are not identical or precisely parallel and . . . it is somatic evolution that is more directly pertinent in taxonomy." As an alternative theory, I would say that evolution is essentially change of some genetic components together with stability of other genetic components, all selected through their phenotypic functions. Part of the controversy arises from Simpson's definition of parallelism. He has not sufficiently considered vestigial characters undergoing parallel regression. Other terms applied to various forms of similarity such as homoplasy, convergence, analogy, and chance seem to me to be adequate. I also agree with his use of interpretive rather than straight descriptive terms.

Simpson defines monophyly as "the derivation of a taxon through one or more lineages from one immediately ancestral taxon of the same or lower rank." To my mind, this definition allows for the inclusion of independently evolved grades of organization or adaptation which are not homologous in the strict (or genetic) sense. For example, he includes the monotremes with the marsupials and placentals under the class Mammalia, although he considers

the common ancestor to be a therapsid reptile and not a mammal. He also includes the superfamilies Ceboidea, Cercopithecoidea, and Hominoidea in the suborder Anthropoidea, although he recognizes the independent origin of the Old World and New World monkeys from a prosimian base. The taxons Mammalia and Anthropoidea as used by Simpson seem to rest upon analogies and diphyletic origins and therefore to be in need of taxonomic revision at the higher category level.

If these differences of opinion were merely arbitrary and concerned the classification and naming of a few groups of animals of interest to a small number of specialists, the matter would not be of great import, but basic principles of biology are involved. Sufficient evidence is available to indicate that more strict definitions of homology, and parallel evolution would provide better correlations of taxonomic and evolutionary order.

Scholarly Approach to Taxonomy

In spite of these criticisms, it is my sincere opinion that this book marks an important advance in taxonomic theory. The result of mature experience in the taxonomy of both fossil and living mammals, it represents a fine scholarly approach to a science that is essential to all comparative biology. Even its ambiguities and inadequacies will set the stage for more critical tests of important hypotheses and interpretations in the near future. All biology will progress as the result of the balanced integration of modern taxonomy within the life sciences.

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Handbook of Abnormal Psychology. An experimental approach. H. J. Eysenck, Ed. Pitman, London; Basic Books, New York, 1961. xvi + 816 pp. Illus. $18.

In this book we have clear indications of how a new, vital discipline has finally evolved into a distinct species and of how it is beginning to find its own place among disciplines of similar genre. As in the case of all historical emergents, it is bound to influence related disciplines in some degree, giving advantage to some and perhaps hastening the modification or disappearance of others as it struggles to survive and grow.

The chapters have their roots in and represent a special focus of experimental psychology. Abnormality is defined not in terms of people suffering from mental disease produced by "definite" causes, but in terms of the defective functioning of various psychological systems. The psychiatric framework is rejected outright. Chapter headings found in textbooks of recent vintage—such as "The neuroses," "Amnesia," "Disordered emotion," "Disorders of volition," and the like—have given way to chapters entitled "Somatic reactivity," "Conditioning," "Learning and abnormal behavior," "Abnormal animal behavior," and "Applied abnormal psychology: the experimental approach." Throughout the book, there is a deliberate effort to avoid the concepts, nosology, and clinical observations of both descriptive and dynamic psychiatry and also, to some extent, the literature on multifactorial tests such as the Rorschach, Thematic Apperception Test, and even the Wechsler intelligence scales. Instead there is a common effort to base all topical reviews on laboratory findings and sound statistical analysis.

The reader, however, should not expect to find many signs of maturity in this young field, apart from some methodological and orientational ones. There is still no body of accepted theory which can come close to unifying the wide, varied literature reviewed.

The theories which are found are primarily those of Hull, Pavlov, and Eysenck. The latter's theoretical formulations are represented out of all proportion to what would be the case if a similar book were compiled in this country, primarily because the authors of the various chapters are mostly his students and colleagues. The treatments of some of the topics are narrower in their outlook than they should be and statements are sometimes offered as fact although they represent still unsettled issues, but the level of the work is uniformly high. Two of the chapters should not have been included at all. The controversial quality of some of the discussions clearly reflects the youth of the subject, but it also indicates the subject's vitality and sense of purpose.

Eysenck asks, "What is a handbook?" And he answers: "A handbook is what a handbook does." What this handbook does is to renounce its psychiatric heritage, to proclaim abnormal psychology as a legitimate offspring of experimental psychology, and to point the direction in which the field must grow. As a single reference and source book of abnormal psychology, it now stands by itself, but before very long we can expect others in this same experimental vein with different emphasis and with more complete development of most of the topical areas.

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Structure and Function of Muscle. vol. 1, *Structure*. 472 pp. $14. vol. 2, *Biochemistry and Physiology*. 593 pp. $16.50. vol. 3, *Pharmacology and Disease*. 489 pp. $15. Geoffrey H. Bourne, Ed. Academic Press, New York, 1960. Illus.

In the not too distant past, the publishing of multivolume handbooks was a specialty of the German scientific world, but it seems that this has now become an American occupation. It is hard to say whether we make them bigger and better, but surely many of them have recently been devoted to various biochemical and other biological subjects, and indeed they form most valuable additions to institutional and departmental libraries.

The work under discussion is not specifically called a "handbook," although it is one because of the breadth of its scope. Its virtues: in three well-executed volumes of not excessive size, it gives a cross section through the field of myology. To various degrees (some special comments follow), the individual chapters are well-rounded and mostly very readable, so that anyone who studies the entire work (which is perfectly possible) will acquire a great deal of knowledge. Its weakness: so much is missing that such an eager reader will still have to supplement his reading to a significant extent if he wishes to be in contact with the major problems, and not all of this additional material is easily accessible. It would have been better, strange to say, if the work had been expanded somewhat to cover some additional topics.

This mild criticism must be substantiated, so let us proceed. There is a lucid over-all review of the biochemistry of muscular action by D. M. Needham and an outstanding chapter on the biochemistry of the sarcosomes by Slater,

but there is no well-developed treatment of the details of the glycolytic pathway and other main aspects of metabolism, no mention of anserine, carnosine, and carnitine, no discussion of myoglobin after its one-word entry on page 1. Meyerhof is quoted several times but briefly; Parnas is mentioned twice, Warburg once, Embden not at all. A basic and brilliant part of modern physiological chemistry is all but omitted. In the field of energetics and dynamics, things are not much different. Hill is quoted frequently, but is often present in name rather than in spirit. There is an original and personal chapter on thermodynamics by Podolsky, and the chapter on biophysics by Ramsey is a heroic effort to pull together some of the essentials. But a broad and systematic treatment of the contributions of the Hill school is missing, just as a treatment of the biochemical foundations laid by Embden and Meyerhof is missing. Has something gone astray?

Some of the omissions are clearly stated by the authors and are deliberate, because of limited space. Thus, Thesleff points out that the study of the effects of drugs on smooth and striated muscle is the largest area of pharmacology, and limits himself to effects related to bioelectrical properties; one chapter similarly introduces the subject of cardiac physiology and restricts the discussion to a few properties of the heart that are closely related to major currents in other areas of muscle physiology. By such restrictions, it has been possible to unite many different topics in a moderate space, but this teaches us that it is, apparently, not feasible to do so without grave omissions. However, we must also note the relative merits of this situation: many of the chapters deal with topics outside the more central or "fundamentalist" currents of muscle research, and these are less likely to be dealt with in other works.

Volume 1 may be the most successful. It is devoted to structure, and the series of chapters on fine structure, introduced in a scholarly treatise by Bennett and continued by several eminent authorities, is as informative as is possible under the circumstances. Volume 2, dealing with biochemistry and physiology, is afflicted most by the omissions referred to, but it contains excellent chapters; one of them, the chapter by A. G. Szent-Györgyi on the proteins of the myofibril, deserves mention for its wealth of well-presented material which is handled in limited space. Volume 3 discusses pharmacology and disease and contains much that deserves the interest of the "pure" scientist.

Yes, this work offers a great deal, and let us make good use of that. Still, I cannot help but feel that among the missing topics are many of the essentials.

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Eskimo Childhood and Interpersonal Relationships. Nunivak biographies and genealogies. Margaret Lantis. University of Washington Press, Seattle, 1960. xv + 219 pp. Illus. $4.75.

This latest publication in the distinguished monograph series of the American Ethnological Society is a collection of 18 brief autobiographical and biographical sketches of Eskimo residents of Nunivak, a small island in the Bering Sea with a total population of about 200.

The life histories were recorded in 1946 by the anthropologist Margaret Lantis, who has been engaged off and on since 1939 in a study of this community. They were obtained with the aid of interpreters and are presented here in English, with editorial commentary by Lantis and with supplementary genealogical information, notes made on a follow-up 10 years later, and psychological interpretations of Rorschach protocols, the last drawn from analyses by Eugenia Hanfmann, a clinical psychologist, and Alice Joseph, a psychiatrist. In the context of Lantis' several earlier publications on Nunivak Eskimo social organization, religion and ceremonialism, and cultural values, this volume contributes detailed information on the way in which the culture of the society has been experienced by some of its carriers and, thus, helps to round out her long-term study of the community.

Specialists in arctic anthropology will need no reviewer's reminder to consult the volume. To nonanthropological scientists, and to anthropologists not specially concerned with Eskimos, the Nunivak life histories present certain intriguing features. One is the evidence of personal strain suffered by members of a small community of arctic hunters in their efforts to satisfy personal wants in culturally conventional ways; this strain is revealed in the complaints recorded in the biographies and in the apparently high suicide and psychosis rate. Another interesting feature is the importance in youthful character formation of participation in community religious rituals, which are frequently mentioned in the autobiographies as significant early memories. And a third major feature is the documentation of the dramatic variability in personal fate of individuals who have lived in what has been a reasonably homogeneous culture. The latter point is worth pondering, for it bears on the question of the "penetrance" of culture: that is to say, on the degree to which knowing the cultural genealogy of the members of a population permits prediction of their life experience and behavior.

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The New Age in Physics. Harrie Massey. Harper, New York, 1960. 342 pp. Illus. $4.25.

Sir Harrie Massey has set himself a task of enormous magnitude, but a task well worth attempting. In a very few pages he presents with skill and great insight a broad view of what is new and important in physics today.

The book begins with a summary of the historical development of our knowledge of atomic structure, which leads to a brief presentation of quantum mechanics and a look at solid-state physics with some of its current applications.

Next are two excellent chapters on special relativity and relativistic quantum theory. The latter may be the most successful part of the book in that it presents ideas far beyond the bounds of "common sense" in a way that should be intelligible to a wide audience.

Then follows a description of the experimental basis of nuclear physics and the theoretical models advanced to explain the observed results. There are two highly relevant sentences in this section which deserve quoting here and which should be displayed in large letters on the wall of every physics classroom: "As always, however, it is the experimental facts which demand the extraordinary interpretation. We are not concerned with speculative philosophy."

The author concludes by describing two experimental techniques of current interest—radio astronomy and artificial satellites. His treatment of satellites is especially valuable since he presents them in proper perspective as remote research stations in otherwise inaccessible regions.

I believe that *The New Age in Physics* will be of great value to those who have some background in classical physics and who have kept their elementary mathematics more or less up to date. Specifically, the book should be most rewarding to scientists in fields other than physics, also to engineers, teachers, physics students, and others who are willing to take the trouble to read with care a book that is densely packed with ideas. This may not be a "popular" book since it would seem to require some previous knowledge of physics. Though not a textbook, it would certainly make excellent supplementary reading in any elementary physics course.

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The Structure of the Eye. Proceedings of the symposium held 11–13 April 1960, during the 7th International Congress of Anatomists. George K. Smelser, Ed. Academic Press, New York, 1961. xv + 570 pp. Illus. $15.

The symposium brought together a large group of international authorities in the fields of the structure and biochemistry of the eye. Electron microscopic, histochemical, immunologic, and biochemical techniques were used to elucidate structural problems. These are not only of significance in ophthalmology but also serve as important model systems: for example, vitreous humor is one of the simplest and most accessible models of mucoid connective tissue; the cornea is one of the simplest and most regular examples of fibrous tissue; the retina can be used as an important model in many phases of neurophysiology; the lack of vascularization of the cornea allows biochemical studies to be made without interference from functions of the blood vessels; proteins of the lens have been used for a long time in studying basic problems of immunologic tissue specificity. In addition to basic structural and physiologic problems, topics discussed include ophthalmic embryology and teratology, radiation biology, and nutritional aspects. This book presents a good cross section of the newer trends in molecular biology and anatomy as applied to ophthalmologic problems.

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Land in British Honduras. Report of the British Honduras Land Use Survey Team. Colonial Research Publications No. 24. D. H. Romney, Ed. Her Majesty's Stationery Office, London, 1959. 2 vols., vii + 326 pp. Illus. + maps. 55s.

This 8800 square mile territory has in this study been divided into 25 subregions, and for each of them the climate, soils, and vegetation have been described and the present land use practice has been analyzed. There are sections on past land use (emphasis on the Maya), land tenure, land forms, and geology; appendixes containing further information on vegetation patterns and lists of plants and animals; and numerous, well-drawn figures and maps. Larger scale maps (1 to 250,000) are contained in a separate folder.

This almost encyclopedic treatment of land and land-people relationships in British Honduras is essential reading for any scholar, businessman, or administrator concerned with that territory.

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New Books

Mathematics, Physical Sciences, and Engineering

Principles of Inertial Navigation. C. J. Savant, Jr., R. C. Howard, C. B. Solloway, and C. A. Savant. McGraw-Hill, New York, 1961. 264 pp. Illus. $9.75.

Principles of Manufacturing Materials and Processes. James S. Campbell. McGraw-Hill, New York, 1961. 683 pp. Illus. $9.75.

Progress in Aeronautical Sciences. vol. 1. Antonio Ferri, D. Kuchemann, and L. H. G. Sterne, Eds. Pergamon, New York, 1961. 289 pp. Illus. $12.50.

Progress in Operations Research. vol. 1. Russell L. Ackoff, Ed. Wiley, New York, 1961. 517 pp. $11.50. First volume in a new series designed to serve as basic reference works. Emphasis in volume 1 is on technical progress in inventory theory, linear and dynamic programming, queuing theory, sequencing theory, replacement theory, simulation, and gaming.

Pure Mathematics. A university and college course. vol. 2, *Algebra, Trigonometry, Coordinate Geometry*. Cambridge Univ. Press, New York, 1960. 447 pp. $6.50.

Relativistic Electron Theory. M. E. Rose. Wiley, New York, 1961. 315 pp. $9.50.

Science in Space. Lloyd V. Berkner and Hugh Odishaw, Eds. McGraw-Hill, New York, 1961. 468 pp. Illus. $7.

A Second Course in Statistics. Robert Loveday. Cambridge Univ. Press, New York, 1961. 166 pp. $1.85.

Sequential Decoding. John M. Wozencraft and Barney Reiffen. Technology Press and Wiley, New York, 1961. 79 pp. Illus. $3.75.

Simplified Calculus. F. L. Westwater. Macmillan, New York, 1961. 175 pp. Illus. $3.50.

Spaceflight Technology. Kenneth W. Gatland. Academic Press, New York, 1960. 380 pp. Illus. $11. Proceedings of the first Commonwealth Spaceflight Symposium, organized by the British Interplanetary Society, 1959.

Tables of the Hypergeometric Probability Distribution. Gerald J. Lieberman and Donald B. Owen. Stanford Univ. Press, Stanford, Calif., 1961. 733 pp. $15.

Tables of ln Γ [z] for Complex Argument. A. A. Abramov. Translated from the Russian by D. G. Fry. Pergamon, New York, 1960. 331 pp. $17.50.

Teach Yourself Atomic Physics. J. M. Valentine. Macmillan, New York, 1961. 192 pp. $1.95.

Theoretical Physics in the Twentieth Century. A memorial volume to Wolfgang Pauli. M. Fierz and V. F. Weisskopf, Eds. Interscience, New York, 1960. 338 pp. $10.

Theory of Elastic Stability. Stephen P. Timoshenko. McGraw-Hill, New York, ed. 2, 1961. 557 pp. Illus. $15.

Thermal Reactor Theory. A. D. Galanin. Translated from Russian ed. 2 (1958?) by J. B. Sykes. Pergamon, New York, 1960. 426 pp. Illus. $15.

Time-Harmonic Electromagnetic Fields. Roger F. Harrington. McGraw-Hill, New York, 1961. 491 pp. Illus. $13.50.

Tools of the Astronomer. G. R. Miczaika and William M. Sinton. Harvard Univ. Press, Cambridge, Mass., 1961. 302 pp. $7.75.

Transcendental and Algebraic Numbers. A. O. Gelfond. Translated from the Russian ed. 1 by Leo F. Boron. Dover, New York, 1960. 197 pp. $1.75.

Transmission of Information. A statistical theory of communications. Robert M. Fano. M.I.T. Press and Wiley, New York, 1961. 399 pp. Illus. $7.50.

Ultrasonics and Its Industrial Applications. O. I. Babikov. Translated from Russian. Consultants Bureau, New York, 1960. 230 pp. Illus. $9.75. Originally published in 1958 as a part of the "Physicomathematical Engineering Library."

Reports

Transport of Gases through Hemoglobin Solution

Abstract. A mathematical theory is developed to explain the observed enhancement of oxygen transport through solutions by hemoglobin. At high partial pressures of oxygen, ordinary diffusion through the solvent accounts for all transport of oxygen, but at low partial pressures the transport may be increased many fold by the presence of hemoglobin. This phenomenon is explained and the possible role of this phenomenon in living organisms is discussed. The theory also indicates a new method of determining dissociation curves from diffusion experiments.

Two recent articles (*1*, *2*) have described several interesting and perhaps important features of gas transport through hemoglobin solutions. Scholander (*1*) and later Hemmingsen and Scholander (*2*) reported observation of an enhanced diffusion of oxygen through solutions containing hemoglobin. Briefly these experiments are reviewed as follows.

A Millipore membrane saturated with a hemoglobin solution separated two gas chambers. In the first instance (*1*) one chamber was maintained at near vacuum while oxygen at a fixed pressure filled the other chamber. In the second case (*2*) oxygen at different pressures was maintained in the two chambers. Similar experiments were performed with nitrogen in place of oxygen. In both cases the flux of gas through the solution-filled membrane was measured.

The results of these measurements revealed that while the nitrogen flux was always simply proportional to the difference in partial pressures of nitrogen in the two chambers, as required by Fick's law of diffusion, such was not the case for oxygen. It was found that the rate of transport of oxygen was proportional to the difference of the partial pressures of oxygen in the two chambers only when the partial pressure in both chambers was appreciable. When the partial pressure of oxygen in the low-pressure chamber was very small, the rate of transport was several-fold greater for the same difference in oxygen partial pressure.

In particular, the first reported results (*1*) in which the low-pressure chamber was maintained at near vacuum showed that the O_2 transport could be represented by the equation:

$$\text{Total } O_2 \text{ flux} = N_2 \text{ flux} \times (0.56) + c$$

where 0.56 is the O_2/N_2 flux ratio in hemoglobin-free solution for the same difference in pressure, and c is a constant. The second report of results (*2*) revealed that this augmentation of transport was not constant, but was reduced by a back pressure of oxygen. Furthermore, the augmentation was shown to be proportional to the hemoglobin concentration of the solution, except at very high concentration.

Scholander (*1*) pointed out that the additional transport mediated by the hemoglobin could be due to the random Brownian motion of the hemoglobin molecules. Thus, a hemoglobin molecule could move from a region of high oxygen concentration to a region of low oxygen concentration. In doing so it would give up some of its associated oxygen which would be taken up by hemoglobin molecules in the region of low concentration. This, of course, would be accompanied by hemoglobin molecules moving in the opposite direction. In this manner a "bucket-brigade" would mediate oxygen transport. That thermal or Brownian motion plays an important role in the transport of oxygen was demonstrated by the fact that oxygen transport was greatly reduced when the solution was solidified with agar gel. Indeed, the theory which has been developed reveals that this is the mechanism of hemoglobin transport of oxygen, as well as of other gases which are associated with hemoglobin.

The theory which has been developed to describe these diffusion phenomenon is presented in detail elsewhere (*3*). It is shown that, for equal differences of partial pressure across a Millipore filter filled with hemoglobin solution, the flux of oxygen $\dot{N}_{O_2}$ and the flux of nitrogen $\dot{N}_{N_2}$ are related by:

$$\dot{N}_{O_2} = \left(\frac{D_{O_2} k_{O_2}}{D_{N_2} k_{N_2}}\right) \dot{N}_{N_2} + \frac{A\phi}{L} nXD_{Hb} \left(1 - S'\,[p_{O_2}(L)]\right) \qquad (1)$$

where D_{O_2} is the diffusion coefficient of O_2 in solvent; D_{N_2} is the diffusion coefficient of N_2 in solvent; k_{O_2} is Henry's law coefficient for O_2; k_{N_2} is Henry's law coefficient for N_2; A is the area of membrane; ϕ is the porosity of membrane; L is the thickness of membrane; n is moles of O_2 per mole of hemoglobin at 100 percent saturation; X is moles of hemoglobin per volume of solution; D_{Hb} is the diffusion coefficient for hemoglobin in solvent; S' is the fractional saturation of hemoglobin; and $p_{O_2}(L)$ is the partial pressure of O_2 on the low-pressure side. (Here p_{O_2} on the high-pressure side is assumed to be large enough to consider $S' = 1$ on this side.) This equation, which is derived from the theory of Brownian motion, shows that the flux of oxygen is a function of oxygen back pressure, $p_{O_2}(L)$. In particular the oxyhemoglobin dissociation curve, S' as a function of p_{O_2}, enters explicitly.

In every respect this theory agrees with the reported experimental observations. Thus the augmentation of O_2 transport is proportional to the hemoglobin concentration, X, up to concentrations great enough to modify the value of D_{Hb}. Effects of pH, temperature, CO_2, and so forth are also implied through the dependence of D_{Hb} and the dissociation curve on such factors. The reported effect of gelation of the solution is also indicated.

Thus, according to the theory of Brownian motion, one has for diffusion of hemoglobin molecules:

$$D_{Hb} \approx \frac{RT}{6\pi\eta r A_0} \qquad (2)$$

where R is the gas constant; T is the absolute temperature; η is the viscosity of solvent; r is the effective molecular radius of hemoglobin; and A_0 is Avo-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to contributors" [*Science* **125**, 16 (1957)].

gadro's number. Hence D_{Hb}, and the augmentation of O_2 transport, decreases with increasing solvent viscosity.

An interesting possibility arising from the theory described here is in the determination of the oxygen-hemoglobin dissociation curve from diffusion experiments. Thus, from Eq. 1

$$S'(p_{O_2}) = \frac{(\dot{N}_{O_2})\ \max - \dot{N}_{O_2}(p_{O_2})}{(\dot{N}_{O_2})\ \max - (\dot{N}_{O_2})\ \min}$$

where the difference in p_{O_2} is always constant, but p_{O_2} on the high-pressure side is always great enough for $S' = 1$ to be assumed, and p_{O_2} in the equation is on the low-pressure side. Such experiments could be performed with a fixed CO_2 partial pressure on both sides of the membrane. This shows that measurements of $\dot{N}_{O_2}$, and p_{O_2} on the low-pressure side, can be used to determine the dissociation curve.

While the theory described has accounted for all features of enhanced transport of gases O_2, CO_2, or CO which associate with hemoglobin, it in itself does not indicate the role of this mechanism within living organisms. Since the dissociation curves for O_2 and CO_2 are similar in form, it is evident that the mechanism is the same for both of these gases within the living organism. In particular transport of gases within the red blood cell should be described by this theory.

A result of the theory described here is that the effective diffusion coefficient for O_2 in a hemoglobin solution is:

$$D_{O_2} = D_{O_2} + nXD_{Hb}\frac{dS'}{dC_{O_2}}$$

where C_{O_2} is O_2 concentration in the solvent. It is evident that in regions of low concentration of the gas in question the transport of the gas is much greater for a given gradient of concentration than in a region of high concentration. This follows since dS'/dC_{O_2} decreases rapidly with increasing C_{O_2}. Thus an important role of this mechanism in living organisms could be to maintain O_2 and CO_2 transport even with lowered gradients of gas concentration. Thus oxygenation of hemoglobin in red blood cells should be more rapid at low oxygen partial pressures in blood plasma. It could also mediate an equitable distribution of gases to tissues, that is, as the concentration of O_2 falls in a given region of tissue the transport of O_2 to that region is spontaneously increased by this mechanism.

In conclusion it should be noted that the theory described here must be modified to include the reaction rate of gas with hemoglobin if the gas concentration in the solution changes rapidly with time.

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27 February 1961

Pollination of Saguaro Cactus by Doves, Nectar-Feeding Bats, and Honey Bees

Abstract. In a large cage, free-flying western white-winged doves, nectar-feeding *Leptonycteris* bats, and honey bees were each effective as cross-pollinators of self-sterile saguaro flowers. Seed production and seed viability were not significantly different in fruit from flowers pollinated by these agents. Pollination is not a limiting factor in saguaro repopulation.

Since the turn of the century the saguaro, or giant cactus [*Carnegiea gigantea* (Engelm.) Britt. & Rose], has failed to repopulate itself in certain areas within the range of its habitat in Arizona (*1*). This report concerns the effect of pollination and of certain pollinating agents on the production of viable seed needed for plant establishment.

The saguaro flower begins to open shortly after nightfall and is usually in full bloom by midnight, when both pollen and nectar are available. The flower usually closes by late afternoon of the same day. Previous work (*2*) showed that the flower may be pollinated both at night and in the daytime and that it is self-sterile and is not wind-pollinated. These findings led to the question: What agents are responsible for pollen transfer?

Preliminary observations indicated that severed saguaro branches (arms), 2 to 5 feet long, would bud, flower, and set fruit satisfactorily. In our experiment 45 arms, removed mainly from windfall plants just before flowering, were placed upright in a 12-mesh plastic-screen cage, 12 by 24 by 9 feet high, located in a cactus forest (*3*).

The prevalence of the day-flying western white-winged doves [*Zenaida asiatica mearnsi* (Ridgway)] during the period of saguaro flowering, and the presence of pollen on their heads, led us to include them as test agents. Six birds of unknown sex were released into the cage (*3*). They soon accepted confinement and were not injured the few times they collided with the plastic screen. These doves fed at will on the nectar in the flowers and on available rolled oats and water.

Saguaro pollen has been found in the stomach and fecal material of nectar-feeding bats (*Leptonycteris nivalis* Saussure) (*4*). This finding, and our observations that pollination may occur at night, suggested that we try these bats. Nine females, three of which were gravid and four with living young, were collected in nearby Colossal Cave (*3*). They were kept in a 12 by 24 by 24 in. screen-bottomed box. Screen was also provided in the ceiling of the container for them to cling to. Because of their sensitivity to excessive temperatures, the bats were kept in an air-conditioned building during the daytime. At nightfall the box with bats was suspended in the cage and gently opened. However, the bats did not break their cluster for an hour or more; they always returned to the box voluntarily before dawn.

Flowering stalks of the century plant (*Agave schottii* Engelm.) were placed in the cage as an attractant for the bats in the early evening before the saguaro

Table 1. Saguaro fruit set, seed production, and seed viability as related to mode of pollination, Tucson, Arizona, 1960.

Mode of pollination	Flowers exposed			Seeds*	
	Dates (inclusive)	No.	Set (%)	Per fruit† (No.)	Germination‡ (%)
Doves	9–18 May	338	44.7§	2634	83.5
Bats	24–26 May	86	61.6§	1887	80.2
Honey bees	27 Apr.–8 May	193	51.8§	1751	90.4
Natural, field-pollinated	8–16 May	132	53.8	2721	83.4
Hand cross-pollinated	26 Apr.–23 May	79	70.9§	2439	91.9
Self-pollinated	26 Apr.–20 June	124	0	—	—

* Data not significantly different at 5-percent level. † Based on counts of seed in ten fruits. ‡ Based on three 100-seed replicates from each of ten fruits. § Includes all fruits harvested plus injured buds that dropped after 7 days.

flowers opened. The yellow pollen from *A. schottii* was found on the stigmas of the saguaro flowers, among the cream-colored pollen from the saguaro, indicating that flowers of both species were visited. There were no claw marks or other signs of damage to the saguaro flowers by the bats. Details of their activity in the cage are described elsewhere (*5*).

Honey bees (*Apis mellifera* L.) have been in the Southwest only since 1872 (*6*), which is not long enough to account for many of the more mature saguaros. However, the bees are attracted to saguaro flowers (*7*) and were included in this test because of their versatility as pollinators.

The bees were housed in a single-body Langstroth hive, well stocked with bees of all stages, honey, and pollen. Water was constantly available. Bee activity in the cage was similar to that in the open (*7*).

Appropriate checks of hand cross-pollinated and of naturally self-pollinated flowers were made intermittently. Such flowers were covered with cheesecloth if pollinating agents were in the cage; otherwise the flowers were left open. For comparison, records were also kept on some naturally pollinated "field" blossoms.

Each day throughout the flowering period all caged flowers were tagged and the fruits that were shed were recorded. As fruits matured they were harvested and the seeds were removed and counted. Germination percentages were determined under conditions of natural light and temperature in the laboratory (*8*).

The results (Table 1) show that viable seeds were produced in considerable quantity by both night- and day-pollinating agents. The effectiveness of the agents tested and the probable effectiveness of similar pollinators in the area (*7*) would indicate that failure of the saguaro to repopulate is not due to lack of cross-pollination.

To our knowledge this is the first experimental proof (*9*) that honey bees can pollinate the saguaro or that white-winged doves and *Leptonycteris* bats can pollinate any plant (*10*).

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3 January 1961

Chemotaxis of Zoospores for Root Exudates

Abstract. A chemotactic response of the zoospores of a soil-inhabiting plant pathogenic fungus, *Phytophthora cinnamomi*, for roots of avocado seedlings was observed. The chemotaxis of the zoospores and chemotropy of their germ tubes were directly related to infection and disease production. Indications were obtained of specificity of the pathogen-attracting root exudate, and interesting implications are evident with regard to mechanisms of invasion and pathogenicity, and to disease resistance.

Plant pathogenic fungi invade their hosts by means of several different avenues including natural openings in host tissue, wounds, and direct penetration of living tissue (*1*). Invasion through such avenues has been attributed to (i) attraction of the fungus mycelium or the germinating spore to stimulatory chemicals exuding from roots or leaves, (ii) attraction of the fungus to a nutrient gradient, or (iii) mere chance.

The complex medium of soil provides a difficult but highly interesting area in which to study attraction of plant roots for plant pathogenic microorganisms. The motile infective zoospores of pathogenic species of the fungus genus *Phytophthora* serve as an excellent tool for investigating such phenomena. Other investigators have demonstrated chemotaxy of motile plant units such as bracken spermatozoids and gametes of *Allomyces* (*2*) to specific chemicals, electric fields, or hormones. Goode (*3*) reported a nonspecific attraction of zoospores of *Phytophthora fragariae* to the root tips and the root-hair zones of strawberry roots. Flentje (*4*) has recently summarized information on means by which plant pathogenic fungi reach their host, and on subsequent invasion of the host.

In the present investigation young, vigorously growing roots of avocado (*Persea americana* Mill.) seedlings were found to have strong attraction for the zoospores of the avocado root pathogen, *Phytophthora cinnamomi* Rands. A preliminary report has been published (*5*). Young excised root tips (1 to 2 cm long, 1 to 2 mm in diameter) from avocado varieties highly susceptible to the pathogen were placed in petri dishes containing actively swimming zoospores of *P. cinnamomi*. Zoospores were liberated from sporangia produced at 24°C for these tests by a method previously described (*6*), involving use of a nonsterile soil extract.

Chemotaxy of the zoospores for the roots occurred within a few minutes and was demonstrated by an obvious accumulation of the motile spores in the immediate vicinity of the root pieces. The concentration of spores was notably greater in the region of elongation on the root, just above the root tip, than it was at the root tip or in the region of differentiation. The avocado root does not produce root hairs.

In 30 to 60 minutes, as a rule, spores settling on and in the vicinity of the roots began to encyst and to germinate. Quantitative evidence of the attraction was then obtained by examining the root pieces under a dissecting microscope and counting numbers of spores settling on different areas of the root and adjacent to the root. These observations showed:

1) That the zoospores were particularly attracted to the region of elongation above the root tip, and that spores also encysted at different distances from the root as if in response to a concentration gradient of some stimulatory chemical exuding from the root (Table 1).

2) Positive chemotropism of germ

Table 1. Attraction of zoospores of *Phytophthora cinnamomi* to roots of a host plant (avocado) and a nonhost plant (citrus).

Distance from root (mm)	Av. No.* of zoospores settling in areas 0.5-mm square
Avocado	
0–0.5	34.0
0.5–1.0	14.7
1.0–1.5	11.1
1.5–2.0	8.7
2.0–2.5	5.0
2.5–3.0	4.2
Citrus (mandarin orange)	
0–0.5	0.6
0.5–1.0	1.4
1.0–1.5	0.9
1.5–2.0	1.3
2.0–2.5	0.9
2.5–3.0	1.3

* Figures represent mean of ten fields counted.

tubes of germinating zoospores for the avocado roots was evident in the case of spores settling on the bottom of the petri dish at distances of up to 2 to 3 mm from the root. The germ tubes were uniformly directed toward the avocado root pieces.

3) Shortly after spore germination occurred, invasion of the root took place through unwounded tissue, and within 24 hours a brown lesion was visible in the region of elongation, identical in appearance to lesions observed in the case of infection of intact plants.

4) Evidence was obtained that the attractive substance is specific to the susceptible living avocado root, since roots killed by boiling or by propylene oxide did not attract zoospores. No chemotaxy of zoospores occurred toward actively growing roots of several other types of plants (tomato, tobacco, mandarin orange) (Table 1). There was also evidence of decreased attraction in the case of avocado varieties with some resistance to *Phytophthora cinnamomi*. Some roots of other plants (macadamia nut, sweet orange, pea) exhibited attraction for the zoospores, but this was primarily to root tips and cut ends of roots rather than to the region of elongation. As further evidence of specificity, zoospores of *Phytophthora citrophthora*, a citrus pathogen, were not attracted to avocado roots, but were attracted to citrus roots (*7*).

5) Zoospores of *P. cinnamomi* and their germ tubes showed chemotactic and chemotropic activity for aqueous extracts of susceptible avocado roots taken up on filter-paper disks. The nature of the substance is under investigation.

These results obviously have interesting implications with respect to resistance and susceptibility of plant roots to pathogens, as well as to various basic aspects of mechanisms of invasions and pathogenicity (*8*).

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3 January 1961

Patterns of Corticosteroid and Pepsinogen Change Related to Emotional Stress in the Monkey

Abstract. In association with conditioned avoidance sessions of 72 hours' duration, monkeys showed a response pattern characterized by increased levels of 17-hydroxycorticosteroids and decreased levels of pepsinogen during the stress period, with a marked and prolonged elevation of pepsinogen levels occurring during the recovery period.

Previous reports from this laboratory have described long-term studies of the effects of behavioral conditioning procedures upon the pituitary-adrenal cortical system in the monkey (*1*, *2*). A major objective in the extension of this work has been the measurement of additional endocrine or visceral functions so that centrally integrated patterns of visceral activity might be brought under investigation. In recent studies concerned primarily with the effects upon adrenal cortical activity of repeated conditioned emotional stress in the monkey, a high incidence of gastric or duodenal ulceration was observed (*3*). These observations prompted us to include, along with determinations of pituitary-adrenal cortical activity, the measurement of pepsinogen levels as a means of indirectly evaluating gastric function in animals under stress. The present report, then, describes a preliminary effort that was carried on to compare plasma 17-hydroxycorticosteroid (17-OH-CS) and pepsinogen responses during and following periods of sustained emotional stress associated with avoidance behavior in the monkey.

Four adult rhesus monkeys, two of each sex, were placed in an experimental chair-type restraining apparatus and allowed several days for adaptation according to previous studies (*4*).

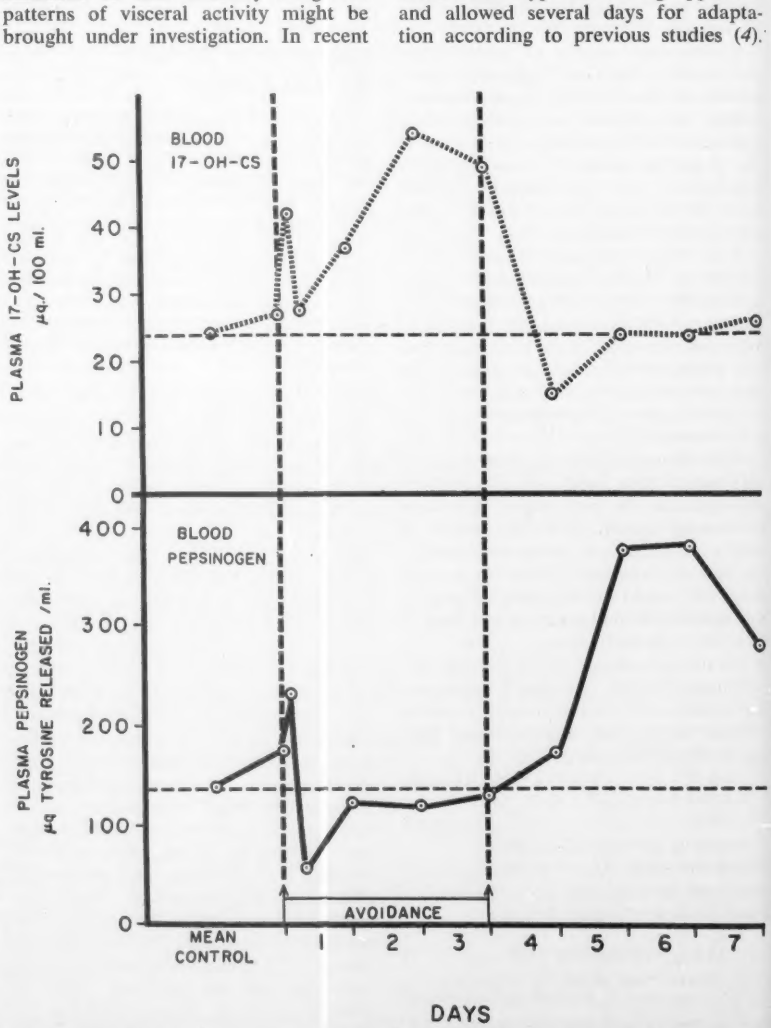

Fig. 1. Mean blood levels of 17-OH-CS and pepsinogen during 72-hour continuous avoidance sessions.

The animals were trained on a Sidman avoidance procedure, in which they must press a hand lever at a moderate, steady rate in order to avoid an electric shock to the feet which, in the absence of lever-pressing, would be delivered automatically every 20 seconds. Even though monkeys learn this procedure well and receive very few shocks, such conditioning sessions are associated with substantial emotional disturbances as evidenced by plasma 17-OH-CS elevations (*2*).

After completion of avoidance training all animals were allowed to sit quietly for a period of 10 days, during the latter part of which control biochemical measurements were made. Then they were subjected to a continuous, 72-hour avoidance session under the conditions described above. In all animals measurements of plasma 17-OH-CS (*5*) and plasma pepsinogen (*6*) were made at 9 A.M. during the final 3 days of the control period, the avoidance period, and a 5-day recovery period. In addition, blood samples were taken 2 and 6 hours after the start of the avoidance session. In one pair of monkeys, 24-hour uropepsinogen excretion (*6*) was followed and in the other pair 24-hour urinary 17-OH-CS excretion (*7*) was measured during the same periods as the plasma measurements.

The mean results of plasma pepsinogen and 17-OH-CS for the four monkeys are shown in Fig. 1. On the first day there is an initial moderate 17-OH-CS elevation, followed by a brief decline, but then a gradual increase in 9 A.M. values during the remainder of the 72-hour avoidance period. In the recovery period there is a rebound depression on the first day, followed by stabilization at pre-experimental baseline levels.

Plasma pepsinogen levels during the first two hours also showed a brief rise, but during the remainder of the 72-hour avoidance period remained below pre-experimental baseline levels. Perhaps the most striking feature of these experiments, however, is the marked, prolonged elevation in plasma pepsinogen levels which develops rather slowly during the recovery period. It is really not until the second morning after the conclusion of the avoidance period that this change becomes fully evident, and levels are still appreciably above the pre-experimental baseline 5 days after the avoidance period.

The associated changes in urinary 17-OH-CS, pepsinogen, and water excretion are summarized in Fig. 2. The urinary findings generally support the blood measurements, indicating a 17-OH-CS elevation and a pepsinogen depression during avoidance, with a prolonged pepsinogen elevation in the aftermath of avoidance. It is also of interest that urine volume tends to fluctuate substantially and in a rather smooth pattern similar to that of pepsinogen excretion although fluid intake was rigidly maintained constant in these animals.

While considerable controversy still exists over the significance of pepsinogen measurements, the fact that both plasma and urinary levels changed substantially and in the same direction in the present experiments would seem to support an interpretation of increased gastric release of pepsin. Other related studies in our laboratory also furnish some evidence in support of this interpretation (*8*). It should be emphasized, however, that firm conclusions regarding gastric changes can be made only with the direct measurement of the gastric secretory rate of pepsin.

The problem of the central regulatory mechanisms underlying the pepsinogen elevation following avoidance is a provocative one, but cannot be settled by our present data. These findings furnished little suggestion that the pepsinogen response could be dependent upon the influence of adrenal cortical hormones, since the two systems have quite different temporal patterns. The prolonged duration of the pepsinogen response also would seem to militate against an explanation based upon an underlying increase in parasympathetic activity. Additional hypotheses which might merit experimental consideration include other hormonal influences, possibly such as growth hormone, which has been shown to elicit marked increases in the secretory capacity of pepsinogen (*9*).

A word of caution may be in order against broad generalizations about gastric responses to other types of stress from this study of only a single type of stressful situation. It is also possible that modified or cumulative effects may develop with many repetitions of conditioning sessions in the same animal. It should be pointed out that the factor of sleep deprivation is an added variable in these experiments and must eventually come under separate evaluation.

In any event, the present data call attention to perhaps two general principles which may be useful in future stress research.

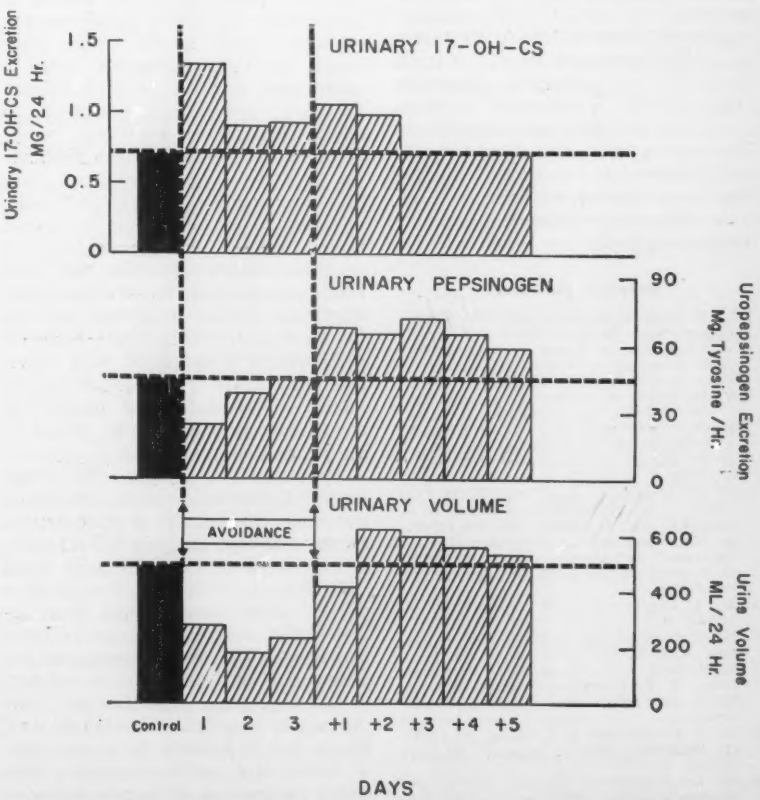

Fig. 2. Urinary volume, 17-OH-CS, and pepsinogen changes associated with 72-hour continuous avoidance sessions.

First, it appears that stressful situations must be considered in their full temporal aspects, and that increased concern with observations during a rather long recovery phase may be particularly profitable. It seems likely that, during the stress aftermath, predominance of regulatory changes associated generally with anabolic events promoting restoration and repair would be appropriate. The possibly critical nature of the temporal patterning of stress versus rest periods in the determination of visceral disorders is also suggested by these data, which indicate that rest periods must be of sufficient duration for the full development of this delayed and prolonged gastric aftereffect.

Secondly, although only two visceral systems were studied, the suggestion is raised that the over-all visceral response to stress may include a much greater variety of regulatory changes than is generally suspected. It appears an important goal in stress research, therefore, to evaluate function in many regulatory systems concurrently so as to test the hypothesis that the responses of these systems are integrated into characteristic and purposeful patterns appropriate to the actual or expected adaptive metabolic needs of the organism (*10*).

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Hormonal Control of Sex Attractant Production in the Cuban Cockroach

Abstract. Virgin females of *Byrsotria fumigata* (Guérin) and several other species of Blattidae produce volatile substances which attract males and release in them characteristic precopulatory behavior. The removal of the corpora allata from females shortly after the imaginal molt results in a failure of production of sex attractant, as assayed by male behavior. Implantation of corpora allata can effect recovery.

The occurrence of sex attractants or sex pheromones (ectohormones) (*1*) in a wide variety of insect species of several orders, and their prominent role in assuring successful mating, are well-known phenomena (*1*, *2*). Their sites of production have been described in some species of Lepidoptera (*2*), and the isolation and chemical identification of two female sex pheromones—those of the moths *Bombyx mori* and *Porthetria dispar*—have recently been achieved (*3*). The physiology of the pheromone receptor organs in the males of several moth species is under investigation (*4*), but little is known of the control of pheromone production in the female.

A clue to possible physiological control was provided by the report of Engelmann (*5*) that a significant percentage of adult females of the cockroach, *Leucophaea maderae*, failed to mate when deprived of their corpora allata 1 day after emergence as adults. Engelmann noted that mating frequently occurred after the implantation of active corpora allata from young last-instar nymphs. Since virgin females of several cockroach species are known to produce sex pheromones (*6*), one may legitimately conclude that such substances are probably of considerable importance in sex recognition and the release of male precopulatory behavior. The present investigation was undertaken to examine the possibility that the failure of allatectomized females to mate might be mediated by failure of sex pheromone production.

To test this hypothesis, the Cuban roach, *Byrsotria fumigata* (Guérin), was used. All females in these experiments were kept singly in 250-ml beakers, the bottoms of which were lined with disks of Whatman No. 2 filter paper. Males were isolated from females and stored in groups in large containers. The testing procedure for pheromone production was as follows.

The filter paper was removed from the beaker containing the female and placed for 2 minutes in a container of males. If a positive response—indicating the presence of the pheromone—was to appear, it nearly always did so within 1 minute. A positive response is signaled by the "wing-raising display," characteristic of the precopulatory behavior of many roaches (*6*, *7*). Actual contact with other males at this time frequently results in the onset of copulatory movements. In the absence of the pheromone, the response is negative; that is, the filter paper is ignored or, at most, casually examined without any evidence of sexual excitement. Tests of this type show that normal virgin females begin to produce the pheromone 10 to 30 days after the imaginal molt and continue to do so for several weeks, sometimes for several months. Females that are permitted to mate show generally a gradual decrease in pheromone production, as indicated by the number of males responding during the tests. Mated or unmated females carrying öothecae rarely produce the pheromone.

To determine whether allatectomized females produce sex pheromone, corpora allata were removed 1 to 3 days after the imaginal molt. These animals, along with the controls, were tested for pheromone production every 5 to 10 days for 30 to 40 days. The results, summarized in Table 1, show that 90 percent of the unoperated controls and 86 percent of the sham-operated controls produced pheromone during the test period, while only 14 percent of the allatectomized individuals did so. Thus, the presence of corpora allata during the test period seems essential for the production of the pheromone.

The correlation between pheromone production and actual mating, as indicated by the presence of a spermatophore in the female's bursa copulatrix, is shown in Table 2. Animals from Table 1 were used in these tests except for an additional group of unoperated controls. After testing each female for pheromone production, a male was added to the female in each beaker, and then each female was examined daily for the presence of a spermatophore in the bursa copulatrix. If mating failed to occur within 3 or 4 days, pheromone production was retested and fresh males

Table 1. Number of *Byrsotria fumigata* females producing sex pheromone. The numbers in parentheses are percentages.

No. tested	Sex pheromone production	
	Presence	Absence
	Unoperated controls	
21	19 (90.4)	2 (9.6)
	Sham-operated controls	
50	43 (86.0)	7 (14.0)
	Allatectomized females	
90	13 (14.4)*	77 (85.6)

*Includes five animals that developed a limited sex pheromone production after the original test period, including the two which subsequently mated (see Table 2).

Table 2. Correlation between sex pheromone production and mating. The numbers in parentheses are percentages.

No. tested	Sex pheromone production			
	Presence		Absence	
	Mating	No mating	Mating	No mating
	Unoperated controls			
30	12	11	0	7
	Sham-operated controls			
23	10	11	0	2
	Allatectomized females			
28	4	6	2	16
	Totals			
81	26 (48.1)	28 (51.9)	2 (7.4)	25 (92.6)

were provided. Nearly all of the 54 females in Table 2 that produced pheromone were "courted" by males immediately after the latter were introduced, whereas this behavior was provoked by only one of the 27 females that failed to produce the pheromone. The high correlation between pheromone production and successful mating is evident in Table 2. In addition, it is noteworthy that the two females that mated in spite of the failure to produce pheromone began to produce small quantities of pheromone some weeks later. Therefore, they may have done so during the test period.

The implantation of corpora allata into previously allatectomized females can result in the recovery of the ability to produce pheromone. In these experiments, each female received by injection into the anterior part of the abdomen four corpora allata taken from adult females 1 to 3 days after the imaginal molt. Two of six animals which received implants 8 to 10 weeks after the imaginal molt showed a strong and sustained recovery of pheromone production beginning 10 and 16 days after implantation. One of these two animals subsequently mated. An older group of 12 animals (implants received 9 to 14 weeks after the imaginal molt) showed no recovery of the ability to produce pheromone. All died 2 to 6 weeks after the operation. Failure to recover may be related to the age of these animals at implantation, for in the absence of corpus allatum stimulation during this prolonged period, the tissue producing the pheromone may have degenerated to such an extent that recovery was no longer possible. Experiments on a series of young adult females (5 to 6 weeks of age) are in progress. Preliminary results show recovery of pheromone production in three of seven animals.

These findings suggest several lines of investigation for future consideration. Of particular interest is the question as to whether pheromone production is directly or indirectly stimulated by the corpus allatum hormone. Preliminary experiments argue against an intermediary role for the ovary, since the removal of the ovaries from last-instar nymphs had no effect on the subsequent production of pheromone. It is also necessary to decide whether pheromone production is the only aspect of the female's mating behavior that is affected by allatectomy. Finally, the results draw attention to the possibility that female sex pheromone production is subject to endocrine control in other species and orders of insects (*8*).

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Nutritional Value of Chemically Modified Corn Starches

Abstract. Male and female weanling rats were fed daily 5 g of a balanced diet supplemented with 1 or 2 g of corn starch, oxidized corn starch, corn starch phosphate, or hydroxyethyl corn starch. Commercially modified corn starches produced the same weight gain as normal corn starch during 21 days of feeding. In contrast to commercial starches, a very highly oxidized laboratory-prepared starch produced a lower weight gain during the same period.

Since increasing amounts of modified starch are being produced to meet requirements for industrial and food uses, it seemed desirable to determine whether slightly modified corn starch might be somewhat more digestible and give higher caloric value than unmodified corn starch.

The nutritional value of starches has been examined by several investigators. Booher *et al.* (*1*) showed that wheat, rice, corn, and waxy maize starches are more digestible in rats than the starches of arrowroot, white potato, and sago palm. Sakurai *et al.* (*2*) found that raw or cooked starches from most cereal grains are effective in producing growth in rats when they are fed as 75 percent of a balanced diet. White potato starch, ball-milled for 250 hours, is equally effective in producing growth, but raw untreated white potato starch is less effective. Jelinek *et al.* (*3*) stated that raw white potato starch is a poor nutritional carbohydrate when fed as 73.6 percent of the diet of weanling rats, but that autoclaved, modified, or ground potato starch is more fully utilized. Booher *et al.* (*1*) showed that modification which produces hydration of, changes the chemical nature of, or disrupts the starch granule makes the carbohydrate a better food substance.

The feeding procedure used was that designed by E. E. Rice *et al.* (*4*) which is based on the theory that food is used for energy to the greatest extent until minimum energy requirements are met. It has been established that the growth of weanling rats varies in proportion to the metabolically available energy of a food when a minimal diet is fed.

Corn starches used as diet supplements were commercial products: corn starch oxidized by 6 percent (wt./wt.) of chlorine, intrinsic viscosity 0.25, carboxyl content 20 meq/100 g of starch; corn starch oxidized by 2.5 percent (wt./wt.) of chlorine, intrinsic viscosity 0.45, carboxyl content 7 meq/100 g of starch; hydroxyethyl corn starch, 0.11 degree of substitution; corn starch phosphate, 0.5 to 0.9 degree of substitution (two samples); and a laboratory sample of corn starch oxidized by 2 equivalents of hypochlorite per D-glucose unit, 43.2 percent (wt./wt.) of chlorine. This very highly oxidized noncommercial starch was prepared only to determine whether

Table 1. Weights gained by rats in 21 days on different types of starch. D.S., degree of substitution.

Type of corn starch	Supplement level	
	1 g	2 g
	Weight gain (g)	
Group 1		
Corn starch	33.7	41.4
a) Corn starch oxidized by 6 percent (wt./wt.) chlorine	33.6	45.0
b) Hydroxyethyl corn starch, 0.11 D.S.	31.6	42.3
c) Corn starch oxidized by 2 eq of hypochlorite, 43.2 percent (wt./wt.) chlorine	18.9	29.7
Group 2		
Corn starch	32.8	43.2
d) Corn starch oxidized by 2.5 percent (wt./wt.) chlorine	35.2	46.8
e) Corn starch phosphate, 0.5 to 0.9 D.S.	36.5	43.5
f) Corn starch phosphate, 0.5 to 0.9 D.S.	33.8	45.2

any effect in weight gain could be produced by feeding a seriously overoxidized product.

Weanling rats (Wistar-Purdue strain) were fed daily 5 g of a highly nutritious basal diet (*5*) supplemented by 1 or 2 g per day of the starch products under investigation. The basal diet (5 g/day) was sufficient to produce a weight gain of 4 to 8 g per week during 4 weeks.

The experiment was conducted in two parts, in each of which nine diets were fed. These were as follows: one basal diet of 5 g/day; four supplemented diets containing 5 g of basal diet and 1 g of a starch product, or a total of 6 g/day; and four supplemented diets containing the basal diet and 2 g of a starch product, or a total of 7 g/day.

The nine diets were arranged in a randomized incomplete block design (*6*), and each diet was replicated four times. Equal numbers of male and female weanling rats were grouped (three litter mates per block, three blocks per replication), individually caged, and fed 5.0 ± 0.1 g per day for 7 days. Water was freely supplied. At the end of this period the animals were weighed and either continued on the basal diet or changed to diets composed of basal diet mixed with 1 or 2 g of starch product supplement. Animals were weighed again after 3, 7, 14, and 21 days of supplementation.

Responses to supplementation were so uniform that 7-day weights reflected the increased caloric intake as definitely as weights at 14 days or 21 days. The weight gains at 21 days for the rats in the two groups of the experiment are presented in Table 1. The data for each group were analyzed in a 4 by 2 factorial design: four types of starch fed at two levels. This anaylsis permitted evaluation of the effect of the amount of supplement, the type of supplement, and the interaction of type and amount.

All the commercial starches produced weight gains similar to corn starch (Table 1). However, starch *c*, heavily oxidized and degraded by 2 equivalents of hypochlorite per D-glucose unit (43.2 percent, wt./wt., of chlorine) had a low nutritional value since the average weight gain produced by feeding this starch was significantly lower than all the others. The average weight gains produced by supplements of 1 and 2 g of this starch were not very much different from that produced by the basal diet (a gain of 26.7 g during 21 days).

The average weight gain produced by all 2-g supplemented diets was significantly greater than that produced by 1-g supplemented diets, the difference being about 10 g per rat in 21 days. There was no interaction between the amount of supplement and type of starch; this is shown by the fact that doubling the amount of supplement increased the gain about equally for all the starch products. There was close agreement between the weight gains reported for corn starch in groups 1 and 2. This similarity allowed a comparison of weight gain between products in the two groups. Heavily oxidized noncommercial starch (*c*) induced diarrhea after the 2nd day of supplementation. Hydroxyethyl starch produced a mild diarrhea.

Autopsies performed on one rat from each of the 2-g supplemented diets disclosed that rats fed heavily oxidized starch *c* had a marked dilation of the colon. The other animals appeared to be normal (*7*).

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Maintenance of Normal in situ Chromosomal Features in Long-Term Tissue Cultures

Abstract. Clonal isolates from a rapidly proliferating fibroblast-like derivative have retained the classic diploid chromosome relationship over a period of many transplant generations. The readily identified members of the 11 pairs of chromosomes, including the sex chromosomes (X_1 and X_2), have aided in localizing minute structural alterations within recloned diploid and aneuploid sublines.

An extensive search to isolate stable diploid tissue cultures of the Chinese hamster (*1*), which continually display the chromosomal state noted *in situ* (*2*), has been wholly successful. The twenty-eighth growth in the series, stemming from adult female fibroblast-like derivatives (FAF-28), possessed the many desired features that characterize the classic diploid state (Fig. 1). The strain was derived from a population of normal cellular components that had infiltrated the peritoneal cavity of an animal bearing tumor CH-38MC, a 3-methylcholanthrene-induced fibrosarcoma. This scheme to initiate cultures was routinely employed, since tetraploid tumor cells always failed to attach to the surface of the flask, leaving normal cellular infiltrates to proliferate for varying lengths of time and, in some cases, without undergoing the eventual shift toward aneuploidy. Generation times gradually shortened as sublines progressed into the tenth month, at which time single-cell cloning trials were conducted in an effort to isolate the increasing number of classic tetraploids. Current parental and clonal derivatives proliferate very rapidly (14 hours or less) in a variety of chemically defined media prepared with whole serum (*3–5*).

Plating efficiencies of three classic diploid and one subdiploid variant, isolated by a modification of procedures described by Puck *et al.* (*5*), ranged from 20 to 40 percent, 7 to 12 days after 60-mm petri dishes were seeded with 500 to 2000 single cells, and in the absence of subsequent and recommended changes of the medium. Precautions regarding serum toxicity and prescribed handling of glassware were neglected during these preliminary trials. FAF-28 exhibits very few spontaneous chromosome breaks. Aneuploidy, that is, a ±1 chromosome deviation from the euploid number, rarely exceeds 20 percent of the cells seen in division, as in the case of intact bone marrow, regenerating liver, and corneal epithelium. Tetraploidy in FAF-28 has fluctuated from 1 to 25 percent of the mitotic population and has consisted primarily of classic tetraploids arising from endoreduplication. It appears that repeated trypsinization of rapidly proliferating classic diploid cells causes dividing metaphases to undergo restitution when suddenly detached and transferred to the new culture flask. Somatically paired homologues are seen more frequently early in the next transfer generation.

Although the later parental FAF-28 sublines were characterized by rigid classic diploidy and tetraploidy, single-cell clonings yielded an unexpected array of aneuploids and even near-triploids. The second cloning trial failed to reveal triploids. On both occasions, classic tetraploid cell types failed to clone. Parental-like classic diploid sublines were repeatedly isolated

by cloning procedures and represented 75 percent (20/26) or better of the clones selected at random. Five other clones (20 percent) featured minor anomalies, such as quasidiploid, hypodiploidy, and hyperdiploidy, and one clone (5 percent) was a near-tetraploid. Thus, the intact or normally observed in vivo pattern of chromosomes was reflected uniformly among in vitro clonal derivatives.

Single-cell cloning of an otherwise classical diploid-tetraploid parental population was demonstrated to be a fertile source from which to isolate cytologic mutants. The ease with which variants proliferated during the clonal period may be a direct response to the greatly reduced numbers of cells employed during cloning and the accompanying release from pressures exerted collectively by large numbers of stem cells. Elimination of the latter may have been instrumental in providing genetically altered forms an opportunity to compete favorably with the sparsely settled classic diploids.

Puck, on several occasions, has commented that plating efficiencies of normal primary explants are exceedingly low and, therefore, inappropriate for use in controlled replication of quantitative studies. The absence of data on plating efficiencies of other long-term euploid cultures of human beings and opossums limits this brief discussion (*6*). Karyological "break-down," resulting in the formation of predominantly aneuploid stemlines (with or without new chromosomes), has been witnessed repeatedly during the course of establishing other strains (*4*, *7*). Similar trends have yet to be noted among the many clonal sublines of FAF-28; the normal *in situ* chromosome relationships are constantly expressed. This is presumably because of the exceedingly rapid proliferative rate of the current and exceptional classic diploid stem cells. When aneuploid or quasidiploid forms predominated in other rapidly proliferating strains, the parental euploid cells were comparatively slow in dividing, thereby encouraging the shift to aneuploidy. One may ask if the rapid proliferative ability of FAF-28 could be accompanied by other undisclosed stem-cell features that reflect greater selectivity when competing with potential mutants. Consequently, single-cell cloning of FAF-28 provides the only opportunity to isolate spontaneous mutation which, otherwise, would be eliminated during routine propagation in the presence of larger numbers of the exceptional euploid stem cells.

Because of these features, FAF-28 is an unusual and rare cell type and one that is highly desirable for experimentation. Its unique features are most applicable in a variety of studies pertaining to somatic cell genetics in vitro.

Parental and clonal sublines of FAF-28 are currently being employed in a series of trials to characterize the responses to x-ray and ultraviolet radiation, production of mutants (genic and chromosomal) by means of selective inhibitory actions of antimetabolites and viral susceptibilities, and to attempt a disclosure of distinctive cytological features that may accompany "malignant transformation," after in vivo implantations (*8*).

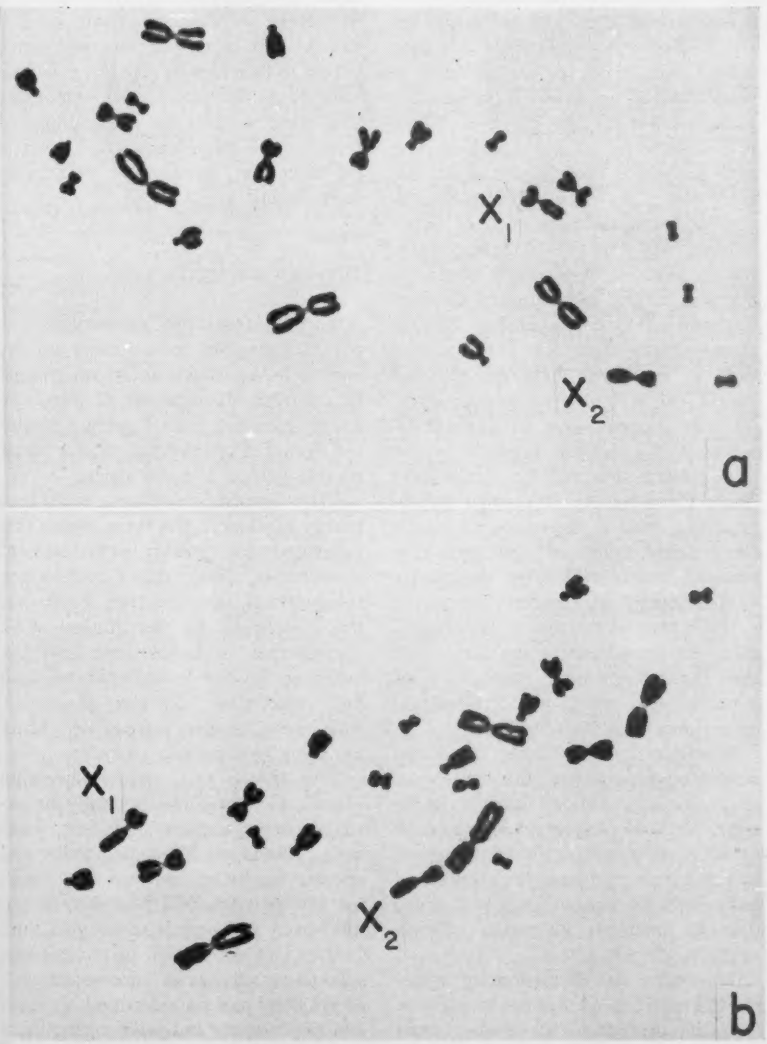

Fig. 1. Classic diploid complement of chromosomes ($2n = 22$) of FAF-28. Note the replicable dual appearance (X_1 and X_2) of the X-chromosome pair, as proposed by the *Triheterosomic* scheme for mammalian sex determination (X_1X_2Y).

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3 January 1961

Ultrahigh-Energy Accelerators

Eventually physicists will push into the domain of superintensity as well as that of superenergy.

Robert R. Wilson

The discovery of the particles of physics and the study of their interactions comes as a culmination to the great developments in quantum mechanics and nuclear physics. This new field, sometimes called high-energy nuclear physics, is characterized by the elegance of both the experiments and the theory. Evolving from the Cambridge tradition of making simple measurements that are directly interpretable, the drive to examine the innermost structure of the nucleus and then of its constituent particles has resulted in the construction of nuclear accelerators of ever-increasing size and complexity. With each order-of-magnitude increase in energy, a new domain of physical experience has been made available. Nuclear structure, nucleon interaction, meson production, strange particles, and antiparticles are but some of the landmarks of this progression. The movement is still developing apace: two 30-Gev proton synchrotrons have just been completed and are beginning to yield most interesting results; a 10- to 20-Gev electron linear accelerator is in an advanced stage of planning; and a 60- to 70-Gev proton synchrotron is under construction.

We are, however, far from a basic understanding of the particles of physics, and this in spite of the rich detail already revealed concerning their properties by excellent experimental and theoretical work. The momentum of the activity already under way, the understanding that this knowledge adds to the stature and dignity of men, the justifiable pride of all concerned in a truly international accomplishment—these considerations and an intense desire and drive to finish what has been started will impel us to continue to work toward an eventual satisfactory understanding of the particles. There is no reason at all for optimism that such an end is near in any sense. Already there has been considerable discussion about the next stage of development. Will it be toward higher intensities of particles at presently attainable energies, or will it be toward the construction of ultrahigh energy accelerators—greater than 100 Gev, perhaps even in the range between 300 and 1000 Gev? Will progress in both directions be necessary—or in neither?

The cost and effort involved in any such construction will be significantly large. Consequently it seemed appropriate to have a discussion in which the broadest points of view were represented and in which the desirability of developing superenergy, from the point of view of the theory of particles, could be considered at the same time that the experimental practicality of constructing and using ultrahigh-energy machines was discussed.

Physicists from all over the world were assembled at the 10th conference on high-energy physics, held at Rochester, N.Y., in August 1960; some 30 or so of these physicists were invited to participate in a completely informal and unofficial session that was given over to intensive discussion of this problem.

We began our discussion by agreeing to keep in mind, but not to mention to each other, that the center-of-mass available energy of a system of two particles in which one is at rest varies as $(2\ ME)^{\frac{1}{2}}$; thus, a beam of 300-Gev protons corresponds to two 12-Gev proton beams opposing one another, and 1000 Gev is equivalent to two clashing 22-Gev beams or to about 44 Gev altogether in the center of mass—as compared to the presently available 7.5 Gev given by the 30-Gev machines.

It was natural to examine cosmic-ray evidence for a clue to new and exciting phenomena, inasmuch as such energies are still not large for the physicist working with cosmic rays. Although nothing of particular interest to the field of nucleon structure is indicated as yet, nevertheless it was felt that this represents inconclusive evidence because the number of events that have been examined has been very small and because the techniques for extracting information from cosmic-ray jets are not such that one could expect to be aware of physical processes that might be considered significant. The cross sections for interesting electromagnetic phenomena are known to be much too small to be determined from cosmic rays.

Ultrahigh Energy Particles

Turning from this not-very-productive exchange, we tried a more positive approach—consideration of what might be desirable about beams of ultrahigh-energy particles. Here I quote a résumé by Robert Oppenheimer of the sense of this part of the discussion.

"The clearest reason for a super-high energy machine is the same reason that motivated the present generation of accelerators, from the Gev electron synchrotrons, the electron linac and the cosmotron to the 30-Gev A.G. synchrotrons in Brookhaven and Geneva: we do not know what we shall find, what finer structure of matter, what new heavier ingredients. There are some new points.

"(1) In the past, cosmic rays were enough to reveal, but not fully to describe, new particles and new processes. This is not happening today and one can hardly be confident that it will, for new particles will probably be too short-lived and new processes too rare.

"(2) Our description of nuclear and subnuclear physics is incomplete, full of arbitrary and ununderstood numbers and parameters, and wide open; there appear to be essential clues that are missing, buried in high energy phenomena. Such are the nucleon 'core,' the

The author is on the staff of the Laboratory of Nuclear Studies, Cornell University, Ithaca, N.Y. Discussants at the informal meeting on ultrahigh-energy accelerators, on 28 August 1960, were as follows: L. W. Alvarez, E. Amaldi, R. F. Bacher, G. Bernardini, H. A. Bethe, B. Blochintsev, N. Bogolubov, G. F. Chew, G. Cocconi, R. L. Cool, M. Danysz, S. D. Drell, R. P. Feynman, D. Glaser, B. Gregory, L. Haworth, W. Heisenberg, W. Jentschke, N. Kemmer, M. S. Kozadaev, T. D. Lee, E. M. McMillan, R. E. Marshak, R. F. Mozley, S. Y. Nikitin, J. R. Oppenheimer, W. K. H. Panofsky, T. G. Pickavance, K. R. Symon, V. I. Veksler, W. D. Walker, and R. R. Wilson.

masses of the 'elementary particles,' the interaction constants themselves.

"(3) Highly unstable heavy particles will probably be found. Stable, or relatively stable, new particles, with new quantum numbers, may be analogous for the baryon-meson system of the μ-meson in relation to the electron.

"(4) Today we do not in any real sense understand the nuclear and subnuclear world. We think it likely that essential novelty will appear at the 'super-high energies' that will promote this understanding. We are confident that a knowledge of what does in fact occur in this domain will take us a long way toward this understanding."

To this we might add a few specific problems, such as the detailed investigation of form factors of particles, determination of the energy at which nucleon and antinucleon cross sections approach each other (that is, what new channels open up at high energy), and study of diffraction disintegration at 100 Gev or more. Also, it might be pointed out that just as the use of high-energy particles has helped clarify many essentially low-energy nuclear problems, so might ultrahigh energy help to shed light on our present problems at high energy.

On the subject of weak interactions T. D. Lee made the following comment.

"We know that if you take the present Fermi theory and extrapolate it to an energy of 300 Gev in the c. of m. [center-of-mass] system it will violate unitarity; it has to be wrong. Exactly at what energy it becomes incorrect is not known. If there is an intermediate boson it will become wrong at a c. of m. energy of about the mass of the boson. If there is no such thing, then the theory will become wrong at some intermediate energy above 100 Mev and below 300 Gev. Now 300 Gev c. of m. energy is very hard to obtain since if you convert it to laboratory energy, it becomes super-super high energy. On the other hand, if the breakdown is not much higher than the order of 10 Gev, then machine energies of the order of several hundred Gev's seem to be quite reasonable."

As to the probability of finding the essential clues to an understanding of subnuclear physics at superhigh energies, it will come as no surprise that the range of opinion between optimism and pessimism is fairly uniformly populated by physicists—but is shaded a bit toward optimism.

Accelerator Design

From these discussions, which seemed to indicate that it would be desirable to have particles of ultrahigh energy, we turned to a discussion of methods of producing such energies. What might be built, with our present knowledge and conventional techniques, with confidence of successful operation? This depends on the cost, of course, and it was generally agreed that for, say, $100 million—or at most $200 million—it would be feasible to push the design of a conventional alternating-gradient proton synchrotron to 100 Gev or even higher, and that this energy level might also cover the first round of experiments. With the same reasoning, but pushing the kind of tolerances that must be held, we could even think of attaining 1000 Gev, and at a cost of less than $1 billion—really a bargain, of course.

Circular electron accelerators seem to be out of the question at hundreds of Gev, so linear accelerators are in order. For these we were given the rough figure of $\$2.5 \cdot 10^6$ per Gev. The disadvantage of the linear accelerator lies in its poor duty cycle and its cost. At high repetition rates the duty cycle is not so bad and might be improved further by cooling the accelerator; furthermore, a new mechanism suggested by Drell indicates that electrons could be very effective in giving highly collimated beams of secondary particles such as pions or kaons. For protons, linear accelerators were felt to be too wasteful of radio-frequency power and too costly to compete with circular machines.

We can expect that some progress will be made in the art of accelerator design in the next several years, the effect of which will be to lower the cost of attaining ultrahigh energies below the costs predicted on the basis of conventional techniques and to reduce the predicted effort. One example of this is the tandem proton synchrotron under consideration now at California Institute of Technology. This uses a small-radius large-gap machine feeding into a large-radius small-gap machine. At the meeting, energy of 300 Gev was mentioned, at a cost of $125 million; however, this estimate was challenged by some, who felt that the accelerator itself does not represent the major part of the cost. This exchange brought out the point of view that, since the cost of the accelerator alone is likely to be about half the total cost of the laboratory, differences of costs between different types should not be taken too seriously. However, isn't it reasonable to look for something that sets the scale of the expense? There is another direction in which technical advances would lead to reduction of cost in accelerator construction—progress toward devices in which the alignment of the magnet is continuously maintained by servomechanisms. The time of traversal of the particles through the large magnets we are now discussing begins to get comfortably long, and thus new possibilities can be considered.

In general, the proton accelerators discussed above can be expected to give intensities ranging from 10^{11} to 10^{12} protons per pulse. K. R. Symon remarked that these intensities can be increased by a factor of at least 100 by using fixed-field alternating gradient methods, but at a cost of 1.5 to 2 times that of more conventional accelerators—a statement that did not go unchallenged by those who felt that the increase in intensity might be closer to 10.

It appears that we can be optimistic about the likelihood of improving conventional accelerators. If that is so, then what is the possibility of a really important innovation being made in the not-too-distant future? No one at the discussion knew of any development giving promise of such success. V. I. Veksler pointed to the possible use of interactions in which many particles collide with one particle, but said that theoretical estimates show this to be quite difficult to achieve. On the other hand, one should not entirely discount the possibility that plasma machines will be developed in the future. Plasma physics is still in its infancy, and developments may occur which could make construction of such machines feasible (*1*). Nevertheless, this possibility should not be given much weight. Less spectacular has been discussion of devices of very high magnetic field, especially those using cryogenic cooling of the coils or even the new superconductors. Although cooling the coils appeared to be an attractive idea at first, it now appears to be beset with difficulties, when the actual numbers and procedures are examined in some detail, as they were in a study recently made at Berkeley. Although we do not see significant innovations emerging at this time, apart from the use of the

new superconductors, we must keep in mind the fact that good ideas have occurred regularly in the past.

Typical, though, of a really new development is the use of storage rings, which, when used in conjunction with a conventional alternating-gradient or fixed-field alternating-gradient synchrotron, could achieve colliding proton beams. Serious work based on this design has been going on for several years. At present it appears that for reasons of cost, design complexity, and experimental convenience, two-way accelerators would not be competitive with a conventional alternating-gradient synchrotron plus storage rings (*2*). It seems unwise to count on the *intensities* available from storage rings ever reaching those of conventional machines; for example, if the injecting accelerator were operating an intensity of $\sim 3 \times 10^{12}$ protons per pulse, and if the injecting and stacking process were carried out with very high efficiency, then a storage ring might equal the rate density of an alternating-gradient synchrotron operating at an intensity of 10^{10} protons per pulse on a liquid-hydrogen target.

For *exploratory* work at very high center-of-mass energies, storage rings appear quite promising, and their relatively low cost makes them attractive even though there are clearly many experiments in which it would be difficult to use them because of their low intensity. For counter experiments, it appears that the intensities which might be available in storage rings would be sufficient to permit detection of new effects or new particles but could never produce intense secondary beams. Furthermore, the secondary reaction products will not be produced at high energy. Inasmuch as colliding-beam devices seem much more limited with respect to the kinds of experiments that can be carried out, and because the technical development is, in fact, yet in its infancy, it seems clear that, for an energy range accessible to both, the conventional accelerator is considerably more desirable.

It was generally agreed at the meeting that, since we can now foresee the possibility of building accelerators with energies up to hundreds of Gev, corresponding to center-of-mass energies of perhaps 20 to 40 Gev, these and not colliding beams might be expected to furnish the next increase in energy. On the other hand, the colliding-beams technique might always be used to extend the center-of-mass energy of any accelerator, and in a really spectacular manner; hence, provision for installation of storage rings should always be considered when a new accelerator is to be constructed. It is unfortunate that the practical development of colliding beams has lagged so far that we do not yet know the limiting factors, and do not yet have the background of experience necessary for proper planning in this respect.

With regard to the question of building machines of superintensity as an alternative to machines of ultrahigh energy, the following might be said. It is perhaps a mistake to think of these as alternatives: both seem to be desirable. One can predict with considerable confidence that the neutrino experiments which can be made by using high energy (10 to 30 Gev) and very high intensities (the required intensity is not yet known) will constitute research into a really new field of knowledge. Exploratory measurements are already being designed, and these will naturally lead on to the next steps. Quite distinct from this is the construction of ultrahigh-energy accelerators, the subject of discussion at the meeting. However, it should be pointed out that production of secondary beams of high intensity may be most readily achieved by going to superenergies.

Experimental Techniques

The discussion livened up considerably as we turned to the question of the experimental techniques that would be available at ultrahigh energy. Some years ago physicists were pessimistic about experimenting with 30-Gev protons. It appeared from the discussion that their fears were not well grounded, inasmuch as the experiments now being made, while not easy, are not very much more difficult than the earlier experiments because of the many advances in technique which have accurred, and most of these techniques would also be useful at superenergies. Among these various advances the following can be mentioned. Cerenkov counters in which a 1-meter path of gas allows for the resolution of $\Delta\beta$ of $4 \cdot 10^{-4}$; spark chambers which have the advantage of responding to a triggered event; bubble chambers in which γ rather than β can be measured by observing the δ-rays from liquid hydrogen (use of a chamber 10 meters long would make this method particularly effective in distinguishing particles, but at not too large values of γ); radio-frequency separators which can be effective up to 20 Gev/c but which will be very expensive; crossed electric and magnetic field devices which can be used to separate particles up to about 10 Gev/c; and storage rings which can serve as particle separators by allowing the most unstable particles to decay (a device especially useful for preparing pure beams of antiprotons).

As the energy gets higher, new methods of distinguishing particles will become practical. For example, the increase in density of a track in a Freon bubble chamber with increasing momentum and, related to this, the lateral extension of the electric field may very probably be applicable to this problem. There is reason to believe that a number of new developments in technique will be made as specific problems in ultrahigh-energy physics arise.

It was, of couse, not possible in a limited time to go far in examining the kind of experiments that might actually be made. Nor was there very much agreement on exactly how to proceed with the experimentation. It was brought out that, in general, a large number of particles will come out in a core of very small angle, as in a cosmic-ray jet. Because much of the energy comes out as invisible π^0's, it would seem no longer possible to determine completely a particular interaction event, as is now possible in a bubble chamber below a momentum of about 1.5 Gev/c. To many present, this seemed no cause for despair. Nuclear physicists have long since learned to disregard what happens to atomic electrons during a nuclear collision. In a similar manner, in superenergy collisions we must learn to distinguish the particles or events with unusual properties from those that are simply obeying the rules of phase space and the laws of physics valid for low-energy phenomena. Distinguishing the very-high-energy particles that come out, or looking at quasielastic collisions were mentioned as examples. Where a particular model is being tested, it may be enough simply to sample average behavior in collisions. Despite the note of optimism on which this last part of the discussion closed (albeit with some mutterings and misgivings on the part of a few of those present), it is clear that particular experiments must be examined in detail to see that meaning-

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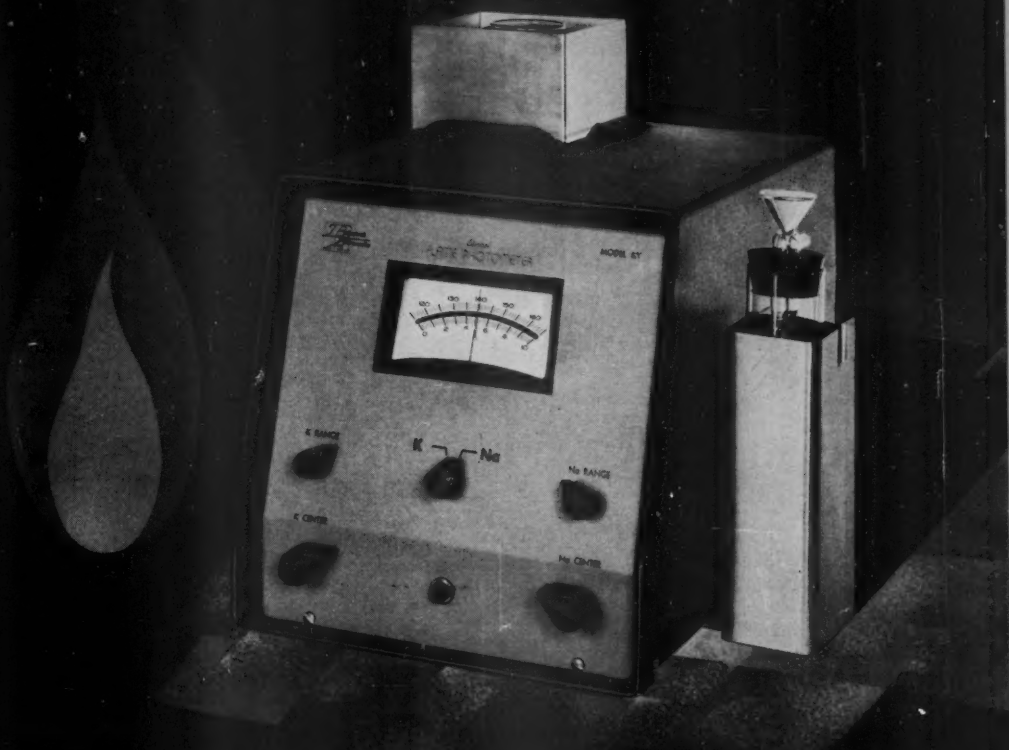

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ful results can actually be obtained. We should be sure that the significant physical processes are not likely to be masked by backgrounds due to uninteresting particles also produced in the interactions.

Summary

Summarizing, one can say that the construction of ultrahigh-energy accelerators will be justifiable even at presently predictable costs. There is little doubt that eventually physicists will push into the domain of superintensity as well as that of superenergy. If they do this, it will be because the information that will then become available will be needed in order to formulate a more complete picture of nature. For the time being, we should examine the results of the 30-Gev synchrotrons in the light of their bearing on these large constructions of the future. But if these projects are to be realized in a time comparable to our lifetime, then those study projects which have become a necessary prelude to actual construction should be started now (*3*).

Notes

1. During a discussion with G. I. Budker concerning plasma instabilities and their deleterious effect on plasma accelerators, I asked if this did not mean we should be very pessimistic about any success. "Yes," said Budker, "but don't forget that a plasma is like a woman, the outlook can change most rapidly!"
2. K. Symon estimated that the cost of producing colliding beams of protons attaining 30 Gev in the center of mass would be about $200 million. The collision yield would be about 10^5 $cm^{-2}sec^{-1}$, with 10 percent gas background at 10^{-8} mm-Hg. G. K. O'Neill says that the cost of a storage ring set might be similar to that of the alternating-gradient synchrotron used as its injector; for example, addition of storage rings to the Brookhaven National Laboratory alternating-gradient synchrotron would cost approximately $35 million, for 60 Gev in the center of mass ("equivalent energy," about 2150 Gev).
3. I gratefully acknowledge the assistance, in preparation of this report, of R. F. Mozley and W. D. Walker, who prepared conference notes, and of Robert Oppenheimer, Gerald Pickavance, and Keith Symon, who sent additional comments and summaries.

Forthcoming Events

June

5–16. Operations Research and Systems Engineering, Baltimore, Md. (Dean, School of Engineering, Johns Hopkins Univ., Baltimore 18)

6–8. Tissue Culture Assoc., 12th annual, Detroit, Mich. (F. E. Payne, Dept. of Epidemiology, Univ. of Michigan, Ann Arbor)

8–11. American Electroencephalographic Soc., Atlantic City, N.J. (G. A. Ulett, Malcolm Bliss Mental Health Center, 1420 Grattan, St. Louis 4, Mo.)

8–18. International Organization for Standardization, general assembly (members only), Finland. (American Standards Association, 70 E. 45 St., New York 17)

9–11. Society of Biological Psychiatry, Atlantic City, N.J. (G. N. Thompson, 2010 Wilshire Blvd., Los Angeles 57, Calif.)

9–17. European Convention of Chemical Engineering, Frankfurt, Germany. (DECHEMA, Postfach No. 7746, Frankfurt/Main 7)

11–15. American Soc. of Mechanical Engineers, summer annual, Los Angeles, Calif. (O. B. Schier II, 29 W. 39 St., New York 18)

12–13. Radio Frequency Interference, 3rd natl. symp., Washington, D.C. (E. F. Mischler, National Engineering Service, Washington, D.C.)

12–14. American Dairy Science Assoc., Madison, Wis. (H. F. Judkins, 32 Ridgeway Circle, White Plains, N.Y.)

12–14. American Neurological Assoc., Atlantic City, N.J. (M. D. Yahr, Neurological Inst., 710 W. 168 St., New York 32)

12–14. Society for the Study of Development and Growth, regeneration symp., Williamstown, Mass. (A. C. Braun, Rockefeller Inst., New York 21)

12–15. Nature of the Real, conf., Milwaukee, Wis. (E. D. Simmons, Dept. of Philosophy, Marquette Univ., Milwaukee 3)

12–15. Physics of Electronic and Atomic Collisions, intern. conf., Boulder, Colo. (B. Bederson, Physics Dept., New York Univ., New York 53)

12–16. Association of Official Seed Analysts, Richmond, Va. (D. D. Forsyth, Agronomy Building, Madison 6, Wis.)

12–16. Molecular Structure and Spectroscopy, symp., Columbus, Ohio. (R. A. Oetjen, Dept. of Physics and Astronomy, Ohio State Univ., Columbus 10)

12–18. European Assoc. for Animal Production, 8th intern. congr., Hamburg, Germany. (European Assoc. for Animal Production, Via Barnaba Oriana 28, Rome, Italy)

12–24. European Inst. of Scientific Studies for the Prevention and Treatment of Alcoholism, Amsterdam, Netherlands. (D. Ehlbeck, Intern. Bureau against Alcoholism, Case Gare 49, Lausanne, Switzerland)

12–29. Statistical Quality Control Intensive Courses for the Chemical and Processing Industries, 18th annual, Rochester, N.Y. (H. M. Kentner, Extended Services Div., Rochester Inst. of Technology, Rochester 8)

13–14. Product Engineering and Production, 5th natl. conf., Philadelphia, Pa. (P. J. Riley, R.C.A., Building 10-6, Camden 2, N.J.)

13–16. Gas Chromatography Symp., 3rd biennial, East Lansing, Mich. (J. E. Callen, Procter and Gamble Co., Miami Valley Laboratories, P.O. Box 175, Cincinnati 39, Ohio)

13–16. Institute of Aerospace Sciences and American Rocket Soc., Los Angeles, Calif. (Inst. of Aerospace Sciences, 2 E. 64 St., New York 21)

13–18. Nuclear Congress, 6th, Rome, Italy. (Ufficio Stampa e Relazioni Pubbliche-CNEN, Via Belisario 15, Rome)

14–16. Applied Mechanics Conf., Chicago, Ill. (American Soc. of Mechanical Engineers, Meetings Dept., 29 W. 39 St., New York 18)

14–16. Semiconducting Compounds, conf., Schenectady, N.Y. (W. W. Tyler, General Electric Research Laboratory, Schenectady)

14–16. Theory of Weak and Strong Interactions, conf., La Jolla, Calif. (T. A. Manar, Scripps Institution of Oceanography, La Jolla)

14–17. American Assoc. of Bioanalysts, Dallas, Tex. (L. D. Hertert, 490 Post St., Room 1049, San Francisco 2, Calif.)

16–17. Meteoritical Soc., Nantucket, Mass. (G. L. Rowland, Long Beach City College, Long Beach 8, Calif.)

17–21. American Nuclear Soc., Boston Mass. (O. J. Du Temple, ANS, 86 E. Randolph St., Chicago 1, Ill.)

18–21. American Astronomical Soc., Nantucket, Mass. (J. A. Hynek, Dearborn Observatory, Northwestern Univ., Evanston, Ill.)

18–23. American Meteorological Soc., 193rd natl., and Pacific Div., AAAS, 42nd annual, Davis, Calif. (AMS, 45 Beacon St., Boston 8, Mass.)

18–23. American Soc. of Medical Technologists, Seattle, Wash. (Miss R. Matthaei, Suite 25, Hermann Professional Bldg., Houston 25, Tex.)

19–21. American Soc. of Pharmacognosy, annual summer meeting, Houston, Tex. (R. S. Westby, Eli Lilly and Co., 740 S. Alabama St., Indianapolis 6, Ind.)

19–21. Space Flight and Re-entry Trajectories, symp. by Intern. Acad. of Astronautics, Paris, France. (Secretariat, IAA, 12 rue de Gramont, Paris 2)

19–23. Conference on Carbon, 5th bi-

ennial, University Park, Pa. (Fifth Carbon Conf., Pennsylvania State Univ., Conference Center, University Park)

19–23. Current Aspects of Internal Medicine, postgraduate course, American College of Physicians, Iowa City, Iowa. (E. C. Rosenow, Jr., Executive Director, ACP, 4200 Pine St., Philadelphia 4, Pa.)

19–24. Feed Microscopy, annual meeting and special short course, Denver, Colo. (C. Jones, Colorado Department of Agriculture, 3130 Zuni St., Denver 11)

19–30. Astrophysics Seminar, Cloudcroft, N.M. (J. R. Foote, P.O. Box 1053, Holloman Air Force Base, N.M.)

21–1. International Plastics Exhibition and Convention, London, England. (British Plastics, Dorset House, Stanford St., London, S.E.1)

22–23. American Rheumatism Assoc., New York, N.Y. (F. E. Demartini, 622 W. 168 St., New York 32)

22–23. Computers and Data Processing, 8th annual symp., Estes Park, Colo. (W. H. Eichelberger, Denver Research Inst., Univ. of Denver, Denver, Colo.)

22–24. Endocrine Soc., New York, N.Y. (H. H. Turner, 1200 N. Walker, Oklahoma City 3, Okla.)

22–26. American College of Chest Physicians, New York, N.Y. (M. Kornfeld, 112 E. Chestnut St., Chicago 11, Ill.)

23–25. American College of Angiology, 7th annual, New York, N.Y. (A. Halpern, Secretary, 11 Hampton Court, Great Neck, N.Y.)

25–28. American Soc. of Agricultural Engineers, annual, Ames, Iowa. (J. L. Butt, 420 Main St., St. Joseph, Mich.)

25–29. Morphological Precursors of Cancer, intern. symp. (by invitation only), Perugia, Italy. (L. Severi, Div. of Cancer Research, Univ. of Perugia, P.O. Box 167, Perugia)

25–30. American Medical Assoc., 110th annual, New York, N.Y. (AMA, 535 N. Dearborn St., Chicago 10, Ill.)

25–30. American Soc. for Testing Materials, Atlantic City, N.J. (R. J. Painter, 1916 Race St., Philadelphia, Pa.)

25–30. International Union of Leather Chemists Societies, 8th congr., Washington, D.C. (F. O'Flaherty, Dept. of Leather Research, Univ. of Cincinnati, Cincinnati 21, Ohio)

25–30. National Education Assoc. of the U.S., Atlantic City, N.J. (W. G. Carr, 1201 16 St., NW, Washington 6)

26–27. Conference on Vacuum Metallurgy, 5th annual conf., New York, N.Y. (R. F. Bunshah, Dept. of Metallurgical Engineering, New York Univ., New York 53)

26–28. American Soc. of Heating, Refrigerating and Air-Conditioning Engineers, 68th annual, Denver, Colo. (J. H. Cansdale, ASHRAE, 234 Fifth Ave., New York 1)

26–28. Control of Noise, symp., Teddington, England. (Director, Natl. Physical Laboratory, Teddington, Middlesex)

26–28. European Symp. on Space Technology, London, England. (Secretary, British Interplanetary Soc., 12 Bessborough Gardens, London, S.W.1)

26–28. Military Electronics, 5th natl. convention, Washington, D.C. (H. Davis, SAFRD, Pentagon, Washington 25)

26–30. American Soc. for Engineering Education, annual, Lexington, Ky. (M. Baker, Univ. of Kentucky, Lexington)

26–30. Concepts and Design in Aerospace Electricity, Philadelphia, Pa. (D. H. Scott, General Electric Co., No. 3, Penn Center Plaza, Philadelphia 2)

26–30. Reading Conf., 3rd annual, Syracuse, N.Y. (R. A. Kress, Syracuse Univ., Syracuse 10)

26–9. Large Dams, 7th intern. congr., Rome, Italy. (U.S. Committee on Large Dams, c/o Engineering Joint Council, 29 W. 39 St., New York 18)

27. Colloid Symp., by Faraday Soc., Glasgow, Scotland. (A. S. Hyde, Chemistry Dept., Royal College of Science and Technology, Glasgow, C.1)

27–29 Analytical Astrodynamics, intern. symp., Santa Barbara, Calif. (Capt. J. L. Gilbert, Air Force Office of Scientific Research, Washington 25)

27–29. Society for Investigative Dermatology, Inc., New York, N.Y. (H. Beerman, 255 S. 17 St., Philadelphia 3, Pa.)

27–30. American Home Economics Assoc., Cleveland, Ohio. (Miss M. Warren, School of Home Economics, Univ. of Oklahoma, Norman)

27–30. Hurricanes, 2nd technical conf., American Meteorological Soc., Miami Beach, Fla. (AMS, 45 Beacon St., Boston 8, Mass.)

28–30. International Gas Conf., 8th Stockholm, Sweden. (R. H. Touwaide, Union Internationale de l'Industrie du Gaz, 4, avenue Palmerston, Brussels 4, Belgium)

28–30. Joint Automatic Control Conf., Boulder, Colo. (R. Kramer, Massachusetts Inst. of Technology, Cambridge 39)

28–1. Institute of Navigation, annual, Williamsburg, Va. (C. T. French, General Precision, Inc., 777 14 St., NW, Suite 611, Washington, D.C.)

29–1. American Assoc. of Physics Teachers, Stanford, Calif. (R. P. Winch, Williams College, Williamstown, Mass.)

July

1–3. Astronomical League, Detroit, Mich. (W. A. Cherup, 4 Klopfer St., Millvale, Pittsburgh 9, Pa.)

2–7. American Physical Therapy Assoc., Chicago, Ill. (Miss L. Blair, Executive Director, APTA, 1790 Broadway, New York 19)

2–9. Rural Medicine, 1st intern. congr., Tours, France. (Prof. Vacher, Secretaire General, c/o Institut National de Medecine Agricole, Ecole de Medecine, Tours).

3–6. Clay Minerals, colloquium on genesis and synthesis of, intern., Paris, France. (Prof. Hocart, Faculté des Sciences, Université de Paris à la Sorbonne, 47 rue des Ecoles, Paris 5)

3–8. Treatment of High Level Radioactive Wastes, symp., Intern. Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

3–16. Durability of Concrete, symp., Intern. Union of Testing and Research Laboratories for Materials and Structures, Prague, Czechoslovakia. (B. Hacar, Director, Inst. of Theoretical and Applied Mechanics, Czechoslovak Acad. of Sciences, Solínova 7, Prague 6-Dijvice)

4–8. Latin-American Assoc. of Physiological Sciences, 4th meeting, Ribeirão

Preto, Brazil. C. R. Diniz, Caixa Postal 301, Ribeirão Preto, Estado de São Paulo)

5–8. European Organization for Research on Fluorine and Dental Caries Prevention, 8th meeting, London, England. (J. R. Forrest, Senior Dental Officer, Ministry of Health, Savile Rd., London, W.1)

5–8. Optical Materials, colloquium, Intern. Commission for Optics, Paris, France. (Institut d'Optique, 3, Boulevard Pasteur, Paris 15)

5–9. International Convention on Radio Techniques and Space Research, Oxford, England. (British Institution of Radio Engineers, 9 Bedford Sq., London, WC.1.)

5–12. International Ophthalmic Optical Congr., London, England. (G. H. Giles, Intern. Optical League, 65 Brook St., London, W.1)

6–7. Free Radicals, intern. symp., 5th, Uppsala, Sweden. (Symposium Secretariat, c/o Inst. of Physical Chemistry, Uppsala)

6–12. Agricultural Medicine, 1st intern. congr., Tours, France. (J. Vacher, Institut National de Medecine Agricole, Ecole de Medecine, Tours)

6–12. Ribonucleic Acids and Polyphosphates: Structure, Synthesis and Function, intern. colloquium, Strasbourg, France. (Prof. Ebel, Faculté de Pharmacie, Université de Strasbourg, Strasbourg)

9–14. Bio-Medical Electronics, 4th intern. conf., New York, N.Y. (H. Schwan, Moore School of Electrical Engineering, Univ. of Pennsylvania, Philadelphia 4)

9–15. American Library Assoc., annual conf., Cleveland, Ohio. (D. H. Clift, 50 E. Huron St., Chicago, Ill.)

9–15. International Dental Federation, 49th annual session, Helsinki, Finland. (Office of Secretary General, IDF, 35 Devonshire Place, London, W.1, England)

10. Bibliographical Soc. of America, Cleveland, Ohio. (E. Wolf II, Library Co. of Philadelphia, Broad and Christian Sts., Philadelphia 47, Pa.)

10–14. Institute in Technical and Industrial Communications, 4th annual, Fort Collins, Colo. (Director, Inst. in Technical and Industrial Communications, Colorado State Univ., Fort Collins)

10–14. International Congr. of Dietetics, 3rd, London, England. (Miss D. F. Hollingsworth, British Dietetic Assoc. 251 Brampton Rd., London, S.W.3)

10–14. International Diabetes Federation, 4th congr., Geneva, Switzerland. (B. Rilliet, Secretary General, 4 Boulevard des Tranchees, Geneva)

10–14. Optical Instruments and Techniques, conf., London, England. (K. J. Habell, Natl. Physical Laboratory, Teddington, Middlesex, England)

10–20. Plant Exploration and Introduction, technical meeting on, Food and Agriculture Organization of the U.N., Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

10–24. Medical Electronics, 4th intern. conf., New York, N.Y. (L. E. Flory, David Sarnoff Research Center, Princeton, N.J.)

11–25. World Meteorological Organization, 3rd South American session, Rio de Janeiro, Brazil. (WMO, 1 Avenue de la Paix, Geneva, Switzerland)

12–18. Radioactivity in Food and Agriculture, Expert Committee on the Organi-

zation of Surveys for FAO, Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

13–14. Data Acquisition and Processing in Biology and Medicine, conf., Rochester, N.Y. (Office of Public Information, Univ. of Rochester, River Campus Station, Rochester 20)

15–18. Life Insurance Medicine, 7th intern. congr., Lisbon, Portugal. (L. de Carvalho Cancella, Secretary, Parede, Portugal)

16–18. British Congr. of Obstetrics and Gynaecology, 16th, Bristol, England. (Secretary, British Congr. of Obstetrics and Gynaecology, University Dept. of Obstetrics, Southmead Hospital, Bristol)

16–22. International Soc. for Clinical and Experimental Hypnosis, Rio de Janeiro, Brazil. (ISCEH, 33 E. 65 St., New York 21)

17–22. Soil Mechanics and Foundation Engineering, 5th intern. conf., Paris, France. (E. Caminade, Secretaire General, 23 rue de Cronstadt, Paris 15)

18–20. Pulmonary Structure and Function, Ciba Foundation Symp. (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

18–21. Inorganic Polymers, intern. symp., Nottingham, England. (General Secretary, Chemical Soc., Burlington House, London, W.1, England)

21–22. World Power Conf. (members only), Moscow, U.S.S.R. (Central Office, 201-2 Grand Buildings, Trafalgar Sq., London, W.C.2, England)

23–28. Otolaryngology, 7th intern. congr., Paris, France. (H. Guillon, Secretary General, 6 Avenue Mac-Mahon, Paris 17)

24–28. Nematology Symp., 6th intern., Ghent, Belgium. (J. van den Brande, Soc. of European Nematologists, Rijkslandboushogeschool, Coupure links 235, Ghent)

24–29. Medical Electro-Radiological Societies, Latin Federation of, 5th congr., Paris, France. (C. Proux, Secretary, 9 rue Daru, Paris 8)

24–30. Urology, 12th intern. congr., Rio de Janeiro, Brazil. (J. Silva de Assis, Secretary, P.O. Box 1275, Belo-Horizonte, Brazil)

26. International Commission for the Prevention of Alcoholism, 7th annual meeting, Washington, D.C. (International Headquarters, 6840 Eastern Ave., NW, Washington 12)

26–28. Detection and Assay of Hormones by Immuno-Clinical Means, Ciba Foundation Colloquium (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

27–1. Macromolecular Chemistry, intern. symp., Montreal, Canada, (Organizing Committee, P.O. Box 816, Sarnia, Ontario, Canada)

28–29. Linguistic Soc. of America, Austin, Tex. (A. A. Hill, Box 7790, University Station, Austin 12)

30–2. Soil Conservation Soc. of America, Lafayette, Ind. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa)

30–3. International Psycholanalytical Congr., 22nd, Edinburgh, Scotland. (Miss C. de Monehaux, 53 York Terrace, Regents Park, London, N.W.1, England)

31–4. Biophysics, 1st intern. congr., Stockholm, Sweden. (B. Lindström, Dept. of Medical Physics, Karolinska Institutet, Stockholm 60)

31–4. Differential Equations in Non-Linear Mechanics, Air Force Acad., Colorado Springs, Colo. (J. P. Lasalle, 7212 Bellona Ave., Baltimore 12, Md.)

31–11. Physics of the Solar System and Re-entry Dynamics, conf., Blacksburg, Va. (Bureau of Public Relations, Virginia Polytechnic Inst., Blacksburg)

31–12. Electric Power and Problems of Nuclear Power, seminar, U.N. Economic Commission for Latin America, Mexico, D.F. (A. Dorfman, Chief, Energy and Water Resource Program, Avenue Providencia 871, Santiago, Chile)

August

1–26. Functional Analysis, 8th American Mathematical Soc. summer institute, Stanford, Calif. (P. D. Lax, AMS, 190 Hope St., Providence 6, R.I.)

2–5. International Conf. of Pure and Applied Chemistry, 21st, Montreal, Canada. (R. Morf, Hoffmann-LaRoche, S.A., Grenzachterstrasse 124, Basel 2, Switzerland)

3–5. Canadian Chemical Conf. and Exhibition, 44th, Montreal. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2, Ont.)

5–9. International Rorschach Soc., 5th congr., Fribourg-en-Brisgau, Germany. (A. Friedemann, Chemin des Pêcheurs 6, Bienne, Switzerland)

6–10. Occupational Medicine and Toxicology, 3rd Inter-American conf., Miami, Fla. (W. B. Deichmann, School of Medicine, Univ. of Miami, Coral Gables, Fla.)

6–12. Atmospheric Ozone and General Circulation, symp., Arosa, Switzerland. (H. U. Duetsch, 20 Carl Spittelerstrasse, Zürich 53, Switzerland)

6–12. Chemical and Thermodynamic Properties at High Temperatures, symp., Montreal, Canada. (N. F. H. Bright, Natl. Research Council, Ottawa, Canada)

6–12. International Congr. of Pure and Applied Chemistry, 18th, Montreal, Canada. (L. Marion, Natl. Research Council, Ottawa 2, Canada)

7–9. Guidance and Navigation Conf., American Rocket Soc., Palo Alto, Calif. (J. J. Harford, ARS, 500 Fifth Ave., New York, N.Y.)

7–9. International Committee of Electro-Chemical Thermodynamics and Kinetics, 13th meeting, Montreal, Canada. (N. Ibl, Eidg. Technische Hochschule, Laboratorium für Physikalische und Elektrochemie, Universitätsstrasse 6, Zürich 6, Switzerland)

7–9. Space Age Astronomy, intern. symp., Pasadena, Calif. (D. W. Douglas, Jr., Douglas Aircraft Co., Inc., Santa Monica, Calif.)

7–10. National Medical Assoc., New York, N.Y. (J. T. Givens, 1108 Church St., Norfolk, Va.)

7–11. High Temperature Chemistry and Thermodynamics, symp., Montreal, Canada. (L. Brewer, Dept. of Chemistry, Univ. of California, Berkeley)

7–11. Seminar on Fast and Intermediate Reactors, International Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

8–11. Poultry Science Assoc., State College, Pa. (C. B. Ryan, Texas A & M College, College Station)

8–16. Society of Protozoologists, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

10–16. International Congr. of Biochemistry, 5th, Moscow, U.S.S.R. (N. M. Sissakian, Leninsky prospekt, 33, Moscow, B-71)

10–16. International Union of Biochemistry, 4th general assembly, Moscow, U.S.S.R. (R. H. S. Thompson, IUB, Dept. of Chemical Pathology, Guy's Hospital Medical School, London, S.E.1, England)

12–19. Fast Reactions, summer school, Cambridge, England. (Secretary of the Summer School, Dept. of Physical Chemistry, Lensfield Road, Cambridge)

13–18. Microchemical Techniques, intern. symp., University Park, Pa. (H. J. Francis, Jr., Pennsalt Chemical Corp., P.O. Box 4388, Chestnut Hill Post Office, Philadelphia 18, Pa.)

13–18. Theoretical Aspects of Magnetohydrodynamics, seminar, University Park, Pa. (Conference Center, Pennsylvania State Univ. University Park)

13–19. International Assoc. of Applied Psychology, 14th congr., Copenhagen, Denmark. (Congress Secretariat, 19 Sankt Pederstraede, Copenhagen K.)

13–19. Training for Research in the Processes of Vision, 1st intern. conf., Rochester, N.Y. (Office of Public Information, River Campus Station, Rochester 20)

14–17. Calorimetry Conf., intern., Ottawa, Canada. (J. E. Kunzler, Bell Telephone Laboratories, Murray Hill, N.J.)

14–19. International Medical Conf. on Mental Retardation, 2nd, Vienna, Austria. (Miss E. Langer, Div. of Maternal and Child Health, State House, Augusta, Maine)

14–19. Symposium on Radiation, Vienna, Austria. (World Meteorological Organization, 1 Avenue de la Paix, Geneva, Switzerland)

14–25. Israel Medical Assoc., 5th world assembly, Jerusalem, Israel. (Beth-Harofeh, 1 Heffman St., Tel-Aviv, Israel)

14–26. Plant Pathology, conf., Lafayette, Ind. (J. F. Schafer, Dept. of Botany and Plant Pathology, Purdue Univ., Lafayette)

14–26. World Eucalyptus Conf., 2nd, São Paulo, Brazil. (Intern. Agency Liaison Branch, Office of the Director General, Food and Agriculture Organization, Viale delle Terme di Caracalla, Rome, Italy)

15–17. International Assoc. of Milk and Food Sanitarians, Jekyll Island, Ga. (H. L. Thomasson, P.O. Box 437, Shelbyville, Ind.)

15–18. Technical Assoc. of the Pulp and Paper Industry, 12th testing conf., Montreal, Canada. (TAPPI, 155 E. 44 St., New York 16)

15–24. International Astronomical Union, 11th general assembly, Berkeley, Calif. (D. H. Sadler, Royal Greenwich Observatory, Herstmonceux Castle, Hailsham, Sussex, England)

16–18. Hypersonics Conf., intern., Cambridge, Mass. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

18–21. Association of American Geographers, East Lansing, Mich. (M. F. Burrill, 1785 Massachusetts Ave., NW, Washington 6)

19–30. Agricultural Economists, 11th intern. conf., Cuernavaca, Mexico. (J. Ackerman, Farm Foundation, 600 S. Michigan Ave., Chicago, Ill.)

20–23. International Ergonomics Assoc., 1st congr., Stockholm, Sweden. (T. Olson, Dept. of Industrial Physiology, G.C.I. Lidingövägen 1, Stockholm)

20–24. American Veterinary Medical Assoc., Detroit, Mich. (H. E. Kingman, AVMA, 600 S. Michigan Ave., Chicago 5, Ill.)

21–23. International Hypersonics Conf., Cambridge, Mass. (F. Ridell, Avco Research Laboratory, 301 Lowell St., Wilmington, Mass.)

21–24. Biological Photographic Assoc., Chicago, Ill. (Mrs. J. W. Crouch, Box 1668, Grand Central P.O., New York 17)

21–24. International Conf. on Photoconductivity, Ithaca, N.Y. (E. Burstein, Dept. of Physics, Univ. of Pennsylvania, Philadelphia)

21–26. International Congr. of Psychotherapy, 5th, Vienna, Austria. (W. Spiel, Lazarettg. 14, Vienna 9)

21–26. World Traffic Engineering Conf., Washington, D.C. (Intern. Road Federation, 1023 Washington Bldg., Washington 5)

21–27. International Assoc. of Dental Students, congr., London, England. (D. H. Clark, Royal Dental Hospital, Leicester Sq., London, W.C.2)

21–27. International Congr. of Zoology, 16th, Washington, D.C. (The 16th Congr., Natl. Acad. of Sciences, 2101 Constitution Ave., NW, Washington 25)

21–31. United Nations Conf. on New Sources of Energy, Rome, Italy. (United Nations, New York, N.Y.)

21–2. International Congr. of Practical Medicine, Merâno, Italy. (Bundesärztekammer, 1 Hädenkampfstrasse, Cologne, Germany)

21–6. Pacific Science Congr., 10th, Honolulu, Hawaii. (Secretary General, 10th Pacific Science Congr., Bishop Museum, Honolulu)

22–25. International Pharmacological Meeting, 1st, Stockholm, Sweden. (A. Wretlind, Karolinska Institutet, Stockholm 60)

22–30. International Conf. on Protozoology, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

23–25. Gas Dynamics, symp., biennial, Evanston, Ill. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

23–26. Electron Microscope Soc. of America, Pittsburgh, Pa. (Miss M. L. Rollins, Agricultural Research Service, U.S. Department of Agriculture, P.O. Box 19,687, New Orleans 19, La.)

23–26. Institute of Management Sciences, 8th annual intern., Brussels, Belgium. (W. Smith, Inst. of Science and Technology, Univ. of Michigan, Ann Arbor)

23–1. Radioisotopes in the Biological Sciences, conf., Intern. Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

24–26. Physiology of the Hippocampus, intern. colloquium, Montpellier, France. (Mme. Mineur, Centre National de la Recherche Scientifique, 13 Quai Anatole France, Paris 7)

26–1. Radiology, 10th intern. congr., Montreal, Canada. (C. B. Peirce, Suite 204, 1555 Summerhill, Montreal 25)

26–2. History of Science, 5th intern. congr., Ithaca, N.Y., and Philadelphia, Pa. (Secretary, 5th Intern. Congress of the History of Science, Cornell Univ., Ithaca)

27–29. International Congr. of Group Psychotherapy, 3rd, Paris, France. (W. Warner, P.O. Box 819, Grand Central Station, New York 17)

27–29. Psychosomatic Aspects of Neoplastic Disease, 2nd annual conv., Paris, France. (L. L. LeShan, Intern. Psychosomatic Cancer Study Group, 144 E. 90 St., New York 28)

27–31. American Soc. of Plant Psysiologists, Lafayette, Ind. (C. O. Miller, Indiana Univ., Bloomington)

27–1. American Congr. of Physical Medicine and Rehabilitation, Cleveland, Ohio. (D. C. Augustin, 30 N. Michigan Ave., Chicago 2, Ill.)

27–1. American Inst. of Biological Sciences, annual, Lafayette, Ind. (J. R. Olive, AIBS, 2000 P St., NW, Washington 6)

27–1. Coordination Chemistry, 6th intern. conf., Detroit, Mich. (S. Kirschner, Dept. of Chemistry, Wayne State Univ., Detroit 2)

28–30. Mathematical Assoc. of America, Stillwater, Okla. (H. L. Alder, MAA, Univ. of California, Davis)

28–30. Oak Ridge Inst. of Nuclear Studies, 8th annual summer symp., Gatlinburg, Tenn. (Symposium Office, University Relations Division, Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

28–30. Scandinavian Symp. on Fat Rancidity, 3rd, Sandefjord, Norway. (E. Törnudd, Gaustadallen 30, Blindern, Norway)

28–31, American Soc. for Pharmacology and Experimental Therapeutics, Rochester, N.Y. (K. H. Beyer, Merck, Sharp and Dohme Research Laboratories, West Point, Pa.)

28–31. Botanical Soc. of America, Lafayette, Ind. (B. L. Turner, Dept. of Botany, Univ. of Texas, Austin 12)

28–31. Chemical Physics of Nonmetallic Crystals, intern. conf., Evanston, Ill. (O. C. Simpson, Argonne National Laboratory, 9700 South Cass Ave., Argonne, Ill.)

28–1. Heat Transfer Conf., intern., Boulder, Colo. (S. P. Kezios, American Soc. of Mechanical Engineers, 29 W. 39 St., New York 18)

28–1. Ionization Phenomena in Gases, 5th intern. conf., Munich, Germany. (Secretariat, Oskar von Miller Ring 18, P.O. 463, Munich 1)

28–1. Radioactive Metrology, symp., Oxford, England. (B. W. Robinson, Applied Physics Division, National Physical Laboratory, Teddington, Middlesex, England)

28–1. Rockets and Astronautics, 3rd intern. symp., Tokyo, Japan. (Japanese Rocket Soc., 1-3, Ginza-Nishi, Chuo-Ku, Tokyo)

New Products

■ AUTOMATIC PARTICLE COUNTER AND SIZER (Fig. 1), manufactured by Casella Electronics Ltd., sizes and counts a wide range of materials either on a standard 3- by 1-in. slide or in a liquid counting chamber. An image of the particles is projected onto an adjustable slit by a conventional microscope system. The sample is mechanically scanned past the slit, and the signals produced by the image of the particle passing over the slit are recorded electronically. Particle size range is 1 to 200 μ. A particle crossing the slit produces a voltage pulse of amplitude proportional to the extent to which the slit is obscured. The reciprocating stage on which the specimen is mounted allows the scanning slit to be located on the optical axis of the microscope. The filament-lamp illumination gives high-intensity and uniform illumination by the Kohler system. Intensity is sufficient to provide a good signal-to-noise ratio for slit sizes less than 1 μ wide.

The number of particles per unit area is obtained by scanning the specimen twice with two slits of different widths but constant sensitivity; "sensitivity" means the length of slit that must be obscured for the particle to be counted. The size distribution of particles is obtained by differentiating the curve of the proportion of particles having sizes greater than specified sizes.

Compensation for variations in opacity of the materials being counted is provided by a gain control. Instead of using the particles themselves to adjust for this compensation, a drum carrying a series of neutral density filters is provided for use once the optical density of the specimen has been determined.

Scanning is automatically performed in two series of 100 passes that bring the specimen back to its starting point but on a slightly offset track. Each traverse is 10 mm long, and the distance between tracks is 50 μ. The total length swept out in this way is 200 cm.

Models are available with up to five amplitude discrimination channels. The use of the multichannel instrument permits a corresponding reduction in the time necessary for accumulation of data required to define a size distribution. (Cooke, Troughton and Simms, Inc., 91 Waite St., Malden 48, Mass.)

Circle 1 on Readers' Service card

■ FLAME ABSORPTION SPECTROMETER is a double-beam instrument with an automatic readout system for the range 2000 to 7700 A. The instrument includes a hollow-cathode lamp source, a source power supply, double-beam source optics, a monochromator equipped with grating, a detector unit with two multiplier-phototube detectors, and associated electronic equipment. Sampling is done by a standard burner with preset regulator, equipped with accessories for burning acetylene, propane, natural gas, or manufactured gas, and with a standard liquid sample nebulizer. To perform an analysis, a liquid sample is atomized in the flame burner. Light from the hollow-cathode source, which must emit the spectrum of the metal being analyzed, crosses the flame and is focused on the entrance slit of the monochromator previously set to read the intensity of the chosen spectral line. The degree of absorption of the source light by the flame is a function of the concentration of the metal in the sample. Typical sensitivities (in μg/ml) quoted by the manufacturer are: calcium, 0.1; magnesium, 0.03; copper, 0.02; zinc, 0.1; cadmium, 0.1; and iron, 0.1. (Perkin Elmer Corp., Norwalk, Conn.)

Circle 2 on Readers' Service card

■ DESALTER for removal of inorganic salts from laboratory solutions prior to chromatographic separation uses ion-exchange membranes. The device includes transformer, half-wave rectifier, potentiometer, and three compartments separated by ion-exchange membranes. The central compartment has a total capability of 6 ml; it is divided into cells of 1, 2, and 3 ml. Outer compartments contain platium-clad electrodes. (Torsion Balance Co., Clifton, N.J.)

Circle 3 on Readers' Service card

■ SEQUENCE REMOTE CONTROL SYSTEM controls the operating time of cameras placed along missile ranges to collect data on test drone flights. The fully

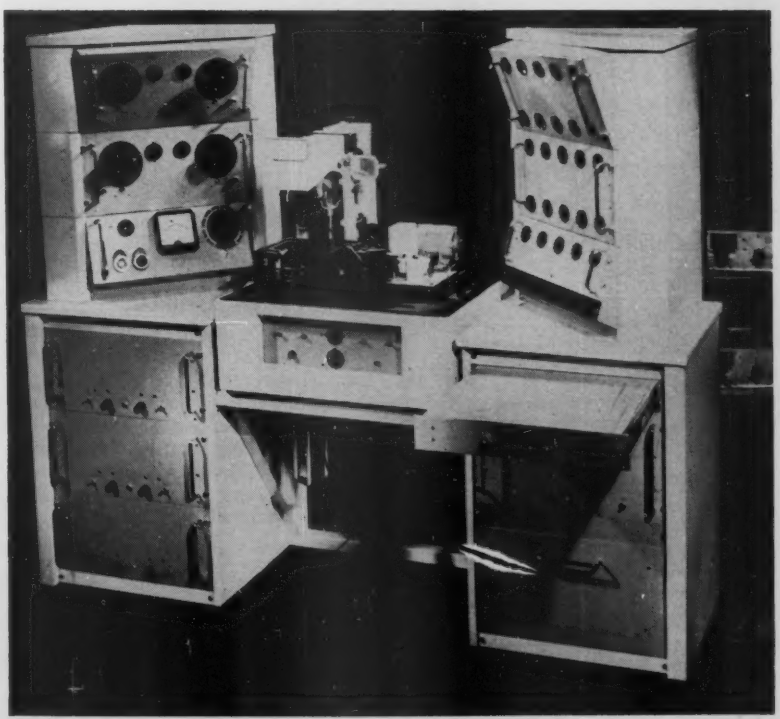

Fig. 1. Automatic particle counter and sizer.

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A Readers' Service card for use in mailing inquiries concerning the items listed is included on pages 1537 and 1627. Circle the number of the items in which you are interested on this card.*

solid-state system is comprised of a console that programs information to launch the drone and to operate any number of cameras in time sequence. This is achieved by presetting a decade counter to the countdown time while additional counters are set to the start and stop times for each camera. All timing is visually indicated, and provision is made for overriding the program (Telemetrics, Inc., 12927 S. Budlong Ave., Gardena, Calif.)

Circle 4 on Readers' Service card

■ DATA CONVERTER (Fig. 2) extracts data from punched tape and writes it on magnetic tape. Conversion of the data is accomplished so that input and output data are completely identical; the output record is a bit-for-bit image of the input tape. A universal code conversion feature is available at extra cost. The system will accept paper, foil, or plastic tapes in widths of five to eight channels. It can write on magnetic tape in formats compatible with the IBM 727 and 729 model 1 and Remington Rand computer inputs. The system includes a paper tape reader capable of reading 120 characters per second, a magnetic tape handler, and necessary electronic circuitry for control and tape format. (Tally Register Corporation, 1310 Mercer St., Seattle, Wash.)

Circle 5 on Readers' Service card

■ ELECTRONIC MULTIPLIER offers single-quadrant multiplication and squaring with accuracy said to be ± 0.01 percent and four-quadrant multiplication accuracy of ± 0.05 percent of full scale. Units are available with two, four, or six channels. Inputs are four independent voltages (x_1, y_1, x_2 and y_2) in the range of ± 100 volts. Outputs are two independent products ($-0.01\ x_1y_1$ and $-0.01\ x_2y_2$) in the range of ± 100 volts at 10 ma maximum load current. Specifications quoted by the manufacturer include: drift, less than 100 mv over an 8-hr period; noise, less than 100 mv, peak; phase shift, less than 1 deg at 100 cy/sec; zero error, with one variable at zero and the other ranging over ± 100 volts, maximum 40 mv. No external power supplies are required for operation with analog computing equipment. (Donner Scientific Co., 888 Galindo St., Concord, Calif.)

Circle 6 on Readers' Service card

■ OPTICAL LEVEL (Fig. 3) manufactured by Hilger and Watts maintains a level line of sight even when its telescope is tilted. The self-leveling device consists of a fixed prism rotating above two swinging prisms. When the telescope is tilted, the suspended prisms change their relation to the axis of the telescope to maintain a level line of

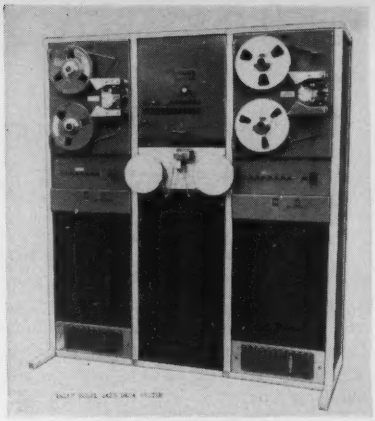

Fig. 2. Data converter.

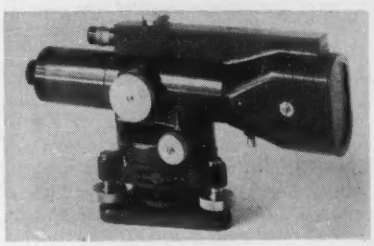

Fig. 3. Optical level.

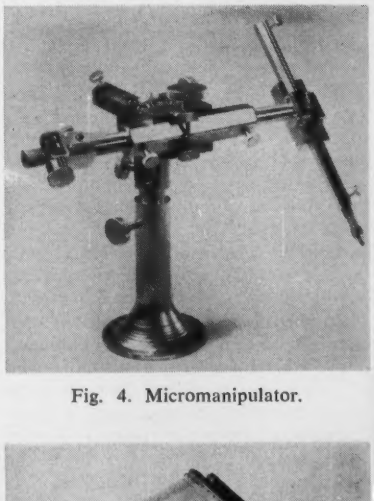

Fig. 4. Micromanipulator.

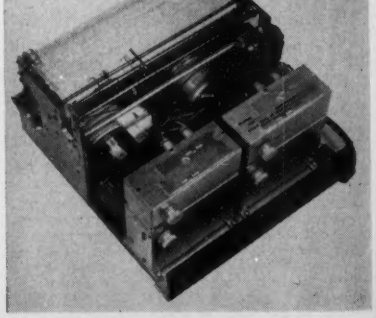

Fig. 5. Integrating strip-chart recorder.

sight. Repetitive setting action is said to be closer than 1 sec of arc. Oscillation of the prisms is prevented by an air-damped piston that also limits swing to ±20 min of arc. The instrument reads directly to 0.001 in. over ½ in. Focusing range is 6 ft to infinity. (Engis Equipment Co., 431 Dearborn St., Chicago 5, Ill.)

Circle 7 on Readers' Service card

■ MICROMANIPULATOR (Fig. 4) is made up of three independent traversing units that may be assembled in any desired combination. Advance of each unit is controlled by a knurled knob directly coupled to a spring-loaded friction wheel that tracks in a groove in the manipulator body. Fine and coarse positioning are independent for each traverse. Motion range is 0 to 125 mm, and sensitivity is said to be 10 μ. Materials of construction are stainless steel and chrome-plated bronze; the entire unit may be autoclaved. (Process & Instruments, 15 Stone Ave., Brooklyn 33, N.Y.)

Circle 8 on Readers' Service card

■ RECORDER-PEN PROGRAMMER, for recorders using solenoid operated pen-lift machanisms, operates the pen lift in five distinct code patterns for trace identification. The programmer is specifically designed for use with antenna pattern recorders and *x-y* recorders when multiple recordings on the same chart are desired. (Scientific-Atlanta, Inc., 2162 Piedmont Rd., NE, Atlanta 9, Ga.)

Circle 9 on Readers' Service card

■ INTEGRATING STRIP-CHART RECORDER (Fig. 5) provides five different maximum count rates, obtained by an adjustable gear system, up to a maximum of 40,000 area counts per minute. Counting rate linearity is said to be ± 0.5 percent of full scale. The integrator channel, installed as a second channel in a standard single-channel recorder, consists of a potentiometer-amplifier-servo arrangement and actuates an area trace simultaneously with the signal trace. Between each peak or at the start of each integration, the integrator pen may be reset to either margin. The integrating circuit may be set to any assumed zero point in the span of the recorder signal. (Texas Instruments, Inc., 3609 Buffalo Speedway, Houston 6, Tex.)

Circle 10 on Readers' Service card

■ MERCURY-VAPOR METERS are offered in three models. Model 21 is a single-range analyzer operating from 0.03 to 3 mg/m³. For operation, the instrument is adjusted to zero in an area known to be free of vapor. Model 23 is a dual-range instrument with ranges 0.005 to 0.1 mg/m³ and 0.03 to 3

mg/m³. The instrument uses two phototubes in a bridge circuit with internal standards to eliminate the need for external calibrating devices. Model 24, similar to model 23, features an audio alarm; alarm concentration is adjustable over the entire range of the instrument. All models operate on 115-volt a-c. (Beckman Instruments, Inc., 2200 Fullerton Rd., Fullerton, Calif.)

Circle 11 on Readers' Service card

■ SAMPLING VALVE (Fig. 6) for chromatographs or mass spectographs has 12 intake ports connected to a common vacuum chamber. Each valve is sealed by a spring-loaded ball except at the time the port is open for sampling. A cam controls opening and closing of each port by moving a rod that unseats the ball. A 3-sec electrical pulse actuates motion of the valve from one position to the next. The valve detents to a new position in 10 sec. (Gelman Instrument Co., 106 N. Main St., Chelsea, Mich.)

Circle 12 on Readers' Service card

■ POTENTIOMETER is a four-dial, six-figure, dual-range d-c instrument with a total range of 2.101010 volts. The manufacturer guarantees accuracy of ± 0.001 percent for a period of 5 years, initial adjustment within ± 0.0002 percent, stability within ± 0.00015 percent per year, thermal electromotive forces less than 0.1 μv, and resolution of 0.1 μv. The potentiometer also functions as a resistance comparator accurate to two parts per million and a saturated–standard-cell comparator that will detect differences of 1 μv. (Sensitive Research Instrument Corp., 310 Main St., New Rochelle, N.Y.)

Circle 13 on Readers' Service card

■ TRACKING FILTER is designed to improve the signal-to-noise ratio of Doppler signals by reducing circuit bandwidth. The device is in effect a bandpass filter whose center frequency automatically tracks the Doppler signal frequency. The tracking bandwidth is adjustable over a wide range by means of a front-panel control even while tracking is being performed. Input frequency-range is 100 to 20,000 cy/sec; input signal level is approximately 3 volts (r.m.s.); tracking bandwidth is adjustable to 1.0, 2.5, 5.0, 10, 25, or 50 cy/sec. (Interstate Electronics Corp., 707 E. Vermont Ave., Anaheim, Calif.)

Circle 14 on Readers' Service card

■ X-RAY IMAGE INTENSIFIER produces an image said to be 3000 times brighter than that available on a conventional fluoroscopic screen. The system accommodates 16- and 35-mm motion-picture cameras or still cameras, either in combination with television or separately. These cameras can be used in conjunction with direct viewing. An automatic brightness control maintains consistent brightness levels when viewing or quickly scanning objects of varying density and thickness. All photographic-exposure factors are automatically compensated, so the operator has only to push a button to record photographically. The intensifier can be used with x-ray generators at voltages up to 250 kv. (Tracerlab Industrial Division, 1601 Trapello Rd., Waltham, Mass.)

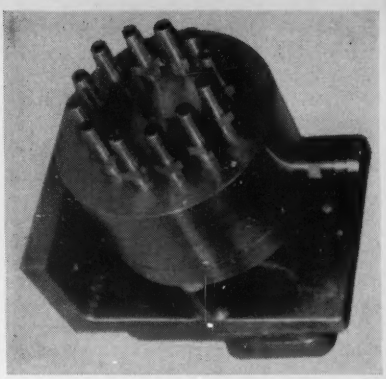

Fig. 6. Sampling valve.

Circle 15 on Readers' Service card

■ RESISTANCE THERMOMETER uses a hermetically sealed platinum resistance element to sense temperature. The sensitive element is 0.138 in. in diameter and 0.4 in. long. Precision of the sensor is said to be better than ± 0.05 ohm or ± 0.05°F; measurement range is $-100°$ to ± 400°F; linearity deviation is said to be less than 1 percent of full range. Nominal resistance is 470 ohms at 32°F; time constant is 2 sec in moving liquid. (Minco Products, Inc., 740 Washington Ave. N., Minneapolis 1, Minn.)

Circle 16 on Readers' Service card

■ MAGNETIC MATERIAL SEPARATOR (Fig. 7) achieves efficient separation by preventing the trapping of nonmagnetic particles within agglomerates of magnetic particles. The mixture to be

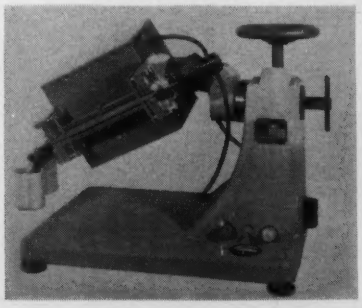

Fig. 7. Magnetic material separator.

separated is fed down a vibrating chute that is inclined sideways as well as forward. An alternating magnetic field is used. Several times in each second the magnetic field is switched off for a few cycles which allows time for the vertical agglomerates to collapse, releasing the nonmagnetic particles and allowing them to migrate toward the lower side of the chute. The magnetic particles are kept toward the higher side by the nonuniform magnetic field that is produced by a current of several hundred amperes flowing in accurately spaced rods that are parallel to the chute. A divider near the lower end conducts the fractions to separate receptacles. (S. G. Frantz Co., 121 Kline Ave., Trenton 6, N.J.)

Circle 17 on Readers' Service card

■ THICKNESS-MEASURING INSTRUMENT measures the thickness of organic and other nonmagnetic coatings on iron and steel. The instrument operates on the magnetic-amplifier principle. The measurement is performed by applying a two-pole probe to the surface and reading a meter deflection. Hardened beryllium copper foils are furnished for calibrating the instrument. Four probes are available with pole separations from 5/32 to 1 in. for thicknesses up to ¾ in. An attachment for applying the probe at constant pressure when measuring soft materials and wires of small diameter is available. (Twin City Testing Corp., Tonawanda, N.Y.)

Circle 18 on Readers' Service card

■ MICROTOME CRYOSTAT provides for mounting a rust-proof microtome in the evaporator. The drive wheel of the microtome is mounted on the outside of the cryostat so that the operator exposes his hands to the low temperature only when introducing or removing the tissue sample. Temperature is maintained between −10° and +20°C. The unit may be operated with the cover open. (International Equipment Co., 1284 Soldiers Field Rd., Boston 35, Mass.)

Circle 19 on Readers' Service card

■ PULSE GENERATOR features rise times of 0.3 nsec, minimum pulse width of 0.7 nsec, and built-in calibrated widths of 5, 10, and 20 nsec. Other pulse widths can be produced with lengths of coaxial cable used externally. Repetition rate is variable from 20 to approximately 300 per second. Pulse generator is a coaxially mounted mercury-wetted switch with an adjustable damping vane for adjustment of undershoot and overshoot. (Lumatron Electronics, Inc., New Hyde Park, N.Y.)

Circle 20 on Readers' Service card

JOSHUA STERN

National Bureau of Standards, Washington, D.C.

Letters

Data on Aging

In the section "Science in the News," *Science* carried an unsigned story [**132**, 604 (2 Sept. 1960)] regarding research done by us. On 19 October 1960 an employee of *Science* signed a receipt for a registered letter which we submitted for publication in reply to this story. You recently informed us, with an apology which we are happy to accept, that our letter was misplaced before it could be printed. Since we do not care to enter the name-calling arena, which is political rather than scientific, we wish, again, to comment about our study and its data.

"A Profile of the Aging: U.S.A." is the first national study of the total life situation of the population 65 years of age and older. Previous national studies have focused on economic status (Steiner and Dorfman), on health and economic status (Shanas *et al.*), or on medical expenditures and medical costs (Odin W. Anderson *et al.*; U.S. Social Security Administration publications). The U.S. Bureau of the Census regularly collects limited data about the total population, which include the "older" category. By contrast, our interviewers asked more questions about religion and religious participation than about health and the economics of health.

We excluded certain groups, chief of which were the recipients of old age assistance grants. Here we followed the precedent of the Social Security Administration, whose 1956 study excluded recipients of old age assistance unless they also received social security payments. It has been estimated that the often-quoted Social Security study *excluded* 55 percent or more of persons 65 and over. Other studies have typically excluded certain categories of the universe to be sampled, and a recently reported national study excluded "individuals in certain occupational groups and those living in institutions."

We are pleased to report that it has been unnecessary to weight any of our data to produce an artificial "representativeness" in our sample. The readers of *Science* will doubtless know that weighting of strata in sample data is a common procedure when the actual sample is found not to be representative of the population sampled. The findings of the U.S. Bureau of the Census are commonly "weighted," particularly in the "Current Population Reports," but also in the "Decennial Censuses of Population." Steiner and Dorfman reported that their data were weighted to compensate for underrepresentation of certain characteristics of the population. A recent report of a joint study by the Health Information Foundation and the National Opinion Research Center (NORC) included weighted as well as unweighted data. We do not wish to be understood as criticizing these weighting procedures. Rather, we invite attention to their being commonplace, and to the high representativeness of our own sample, which made weighting unnecessary.

Characteristics of our sample are compared to independent estimates of the United States population 65 years of age and over in Table 1. It should be noted that the sample was not stratified for these characteristics, and that the data shown for the "Profile" study are purely random.

The readers of *Science* will be familiar with a number of procedures for analyzing the "fit" of the two sets of characteristics.

Considerable attention has been given to our findings, with the statement or inference that they are inaccurate. As a matter of information only, it can be reported that the findings of the Steiner-Dorfman study were called "controversial." Ethel Shanas's National Opinion Research Center study also created considerable discussion. Her report of income for the aged was higher than U.S. census estimates, and she reported that 60 percent of the aged are either as well off economically after age 65 as before, or are better off after 65. In spite of the generally recognized fact that census figures for income are some 20 percent too low, Shanas's findings were attacked again in the "Background Paper on Income Maintenance," prepared for the 1961 White House Conference on the Aging. (It seems fairly obvious that the Social Security study would tend to substantially understate income, since the recipients of Social Security retirement grants are removed from the rolls if their income from employment rises too high.)

From the latest data available, it is illuminating to examine the income of the aged. The "Chart Book" for the White House Conference on Aging states that federal programs provided $17 billion in benefits and services to the aged population. The "Background Paper on Income Maintenance" reported that the federal programs provided between one-third and two-fifths of the total income to the aged. Assuming the lesser total income, we reach a gross income for 17 million aged of $42.5 billion. Simple arithmetic reduces this to an average per capita income of $2500. The median aged respondent in the "Profile" study reported income between $2000 and $3000.

Our findings in the field of health produced some comment. We found that 90 percent of our respondents had no unmet medical needs that they knew of. It has been suggested that all kinds of people know more about an older person's health than he does. In any case, a considerable number of studies by state or region, and most national studies, have assumed that the respondent has a fair idea whether he is sick or not.

Table 1. Random characteristics of the "Profile" study sample compared with data from other sources.

"Profile" study		U.S. census*	NORC†
Category	%	(%)	(%)
Age distribution			
65–69	34.5	37.3	
70–74	26.5	28.0	
75–79	22.3	19.3	
80–84	11.5	9.8	
85 and over	4.3	5.3	
Marital status			
Married	54.0	51.9	
Divorced	3.0	1.5	
Widowed	35.4	38.1	
Single	6.4	7.2	
Separated	0.4	1.3	
Not married	46.0	48.2	
Religious preference			
Protestant	74.5	67.9	
Catholic	19.0	22.2	
Jewish	4.7	3.7	
Other	1.7	1.3	
Sources of income			
Employment	31.4		30.4
Old age and survivors insurance	58.8		57.3‡
Rent	20.0		17.8
Non-cash assistance	32.1		30.8

* Age distribution data for 1957; marital status data for March 1959 [*Current Population Repts. Ser. P-20, No. 96* (1959)]; religious preference data for 1957 [*Current Population Repts. Ser. P-20, No. 79* (1958)]. † Data for 1956. ‡ Includes related programs.

Ninety-six percent of our respondents reported no medical debts, and exactly the same percentage was found by Steiner and Dorfman for 1951.

The most recent study of medical expenses of the aging known to us is based on data collected through the National Opinion Research Center. Odin W. Anderson, Patricia Collette, and Jacob J. Feldman, in "Family Expenditures for Personal Health Services" (Health Information Foundation, 1961), present findings comparable to our own. The "Profile" study showed that 97 percent of respondents had expenditures for physicians below $50 for one month, and that 2 percent had expenses above $50 but below $100. Anderson *et al.* found that 86 percent of their aged respondents had expenditures for physicians below $100 for an entire year. The "Profile" study showed that 95 percent of the respondents had no hospital expenditures in one month and that 3 percent had hospital expenditures below $100. Anderson reports that 86 percent of his aged respondents had no hospital expenditures in a year, and that 5 percent had hospital expenditures below $100. According to the "Profile" study, 98 percent of the aged had expenditures for medicines of less than $50 in a month, while Anderson reported that 88 percent had spent less than $100 for (prescribed) medicines in a full year.

If a few of our regional associates in the study, in response to a request from a subcommittee of the United States Senate, have felt it their duty to support the subcommittee, we may expect the data to be biased in favor of universal misery. If, in spite of the data they delivered and certified to us, some associates wish to believe that the aging are in a grave plight, it is a tribute to their professional competence and scholarly objectivity that they furnished the data as obtained by the interviewers. It has often been said that a chief mark of the scientist is that he even reports findings he does not like.

JAMES W. WIGGINS
HELMUT SCHOECK
Emory University, Atlanta, Georgia

Our reporter did try to contact Wiggins and Schoeck before publishing the news article. He telephoned Atlanta, but was unable to reach them. His report was based not on the press releases of the Senate subcommittee but on an examination of the letters in the files of the subcommittee; interviews with American Medical Association officials in Washington; the report, under the by-line of Wiggins and Schoeck in the Wall Street Journal *summarizing the findings of their study; and the A.M.A. press release interpreting their work.*—ED.

Degrees and Titles

This letter is a commentary on your most interesting editorial in *Science* [**133**, 441 (17 Feb. 1961)] entitled "A question of degrees." In 1920 the Society for the Rationalization of the Title of Doctor was organized at the University of Virginia and immediately received a great deal of favorable publicity. I would like to call your attention to the stand the society took at the time, but I have to rely on my memory alone. I believe the following numbered statements give the society's position.

1) The title of Doctor was to be limited to doctors of medicine, dentists, druggists, ministers of the Gospel, and Ph.D.'s of less than 1 year's standing, although, on occasion, it could be applied to a Ph.D. in either affection or derision.

2) The title of Professor was to be limited to high school teachers (male), to aviators giving exhibitions (they did in those days), and to any professional wrestler who owned a gymnasium and taught wrestling.

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3) Any student who called a member of the faculty "Professor" was to be corrected on the spot.

4) All members of the faculty were to be called "Mister" (there were no female members), unless they taught in the Medical School. In this latter case it was just too much trouble to tell the M.D.'s and the Ph.D.'s apart, so they were lumped together and called "Doctor."

5) The president of the university should never be addressed as "Your Excellency."

The above standards have the virtue of being precise and definite. I admit that sometimes I am in doubt about the proper way to address a colleague—whether to call him "Doctor" or "Professor." I fear I take the easy way out and compromise. When in doubt I address him as "Colonel."

CONWAY ZIRKLE
Botanical Laboratory, University of Pennsylvania, Philadelphia

Your editorial "A question of degrees" was a timely one. I was surprised, however, that you failed to take a stand on the issue. Members of the medical profession have been only too eager to appropriate the term *doctor* for themselves and fully exploit its value (even medical students are called "Doctor" in hospitals), and, due no doubt to various group pressures, the Washington *Post* has been following a highly discriminatory editorial policy under the very eyes of professional societies. Were it not for the lack of intelligent action by professional societies and for the timidity of some Ph.D.'s, the "question of degrees" would probably not have arisen in this country.

Incidentally, most surgeons and physicians in England are called "Mister" because, very simply, they do not have a doctorate. The minimum requirement for the practice of medicine in England is a Bachelor of Medicine degree similar to our Bachelor of Laws degree; the British M.D., which is higher than an American M.D., requires the completion of original research work and proof of many years of professional competence.

STEPHEN D. BRUCK
4401 East West Highway, Bethesda, Maryland

In your editorial "A question of degrees" you write, "The degree of doctor of philosophy was . . . modeled after the German Ph.D."

Some information, almost certainly well known to you, that you could have presented in the last half of your editorial is that common courtesy, almost anywhere in the world, indicates that an individual is most appropriately

addressed by his highest title. For example, the president of a university who once was a professor and who also had been granted a Ph.D. degree would be addressed in Germany as "Herr President," and if it was needful to repeat all of his titles, they would appear as "Herr President Professor Doctor." Discerning persons notice the impropriety of addressing a given individual as "Doctor" instead of "Professor" if he is entitled to both titles—provided, of course, there is intent to follow the German system.

The English title of Mister depends on matters nonacademic.

In summary, the problems that you pose might be answered in one way if the German system were followed and in another way if the British system were followed. It seems to me that there is no American system. If there is an American system, perhaps someone like you who has given thought to the matter ought to outline it. Those of us who are teachers might find it useful to have a recommended system in order to teach students in American universities how to avoid unintentional discourtesies.

E. RAYMOND HALL
Museum of Natural History, University of Kansas, Lawrence

Loyalty Oath

Jack P. Hailman's letter regarding the loyalty oath [*Science* **133**, 251 (27 Jan. 1961)] reawakens a grave concern regarding the growing tendency of Americans (not only scientists) to shrink from an opportunity to reaffirm love for, belief in, and loyalty to their country. This tendency is approaching a stage of neurosis, or negative thinking, in which a loyalty oath is regarded as being as surely preliminary to adoption of the cloak of the Fifth Amendment as a Bach toccata is indicative of an impending fugue. Such concern was in no way allayed by the eloquent appeal of Bentley Glass on behalf of the resolution, adopted by the AAAS Council at the Chicago meeting in 1959, recommending elimination of this requirement for the grant of National Science Foundation fellowships.

I would suggest to Hailman that he might ponder whether a President-elect of the United States should feel, concerning the not dissimilar oath he is required to take at his inauguration, "How unnecessary!" It seems to me that, if one is loyal to his country, taking such an oath is the least undertaking he can make, and rather than regard it as an insult, he might better be willing to take the oath at every available opportunity.

On moral grounds it can be ques-

tioned whether falsely subscribing to an oath can be regarded as an "insignificant offense," even in a comparative sense. And it matters little whether the oath is made in reference to the Bible, the Koran, or the Talmud. If one is a believer in the sanctity of oaths, as one would expect a believer in American ideals to be, there can be no crossing of the fingers, no seeking of special immunity for the field of science. Not to acknowledge the sanctity of oaths is to flirt with the moral dangers of agnosticism and with social beliefs inimical to the Western world.

Let us remember that disbursements of funds by the National Science Foundation are largely disbursements of funds of all American citizens, who have an essential interest—too often disregarded, one might add—in the manner in which such funds are used. But the most publicized defections from the Western world are those of people with access to scientific knowledge which could be useful to unfriendly powers. Every citizen should reasonably require that his funds be disbursed in such a manner as to bring maximum benefit to his country. The loyalty oath is certainly a means of *trying* to ensure this. The average citizen might well feel that it should be required of the scientist above all, in view of past happenings and of the scientific revolution which he is told he is witnessing.

If one were to require every person in the country who is to benefit from federally financed programs of any kind to take a loyalty oath—a course suggested by Hailman as being less objectionable—we would require it of every citizen from womb to tomb. That might indeed be desirable, but would it be practicable? In naturalization proceedings, for example, the courts normally absolve those of tender years from taking the loyalty oath. Perhaps it should be regarded as acknowledgment of maturity that graduate students are required to take the oath. They are, naturally, free to decide whether national funds available to them are worth a moral commitment.

One might echo President Kennedy's rhetoric, "Ask *not* what your country can do for you. Ask what you can do for your country!" The need for good scientists is freely acknowledged, but "good" has many connotations. All of them are implied in this context.

I should not like to think that the requirement of a loyalty oath for National Science Foundation fellowships is, through individual decision or the counsel of others, depriving us of sound scientists. I cannot feel that the requirement is depriving us of *good* sound scientists.

I hope that, if time permits, Hailman will reconsider his decision and take the oath, which would not deprive him of any rights but which would, in some eyes at least, enhance his stature as a *good* sound American scientist.

A. J. HAWORTH

30 Glendale Drive,
Glenbrook, Connecticut

UNESCO Statements on Race

If there is anything less profitable than replying to a hostile reviewer [see *Science* **133**, 873 (24 Mar. 1961)], it is to consume the valuable space of a journal devoted to more edifying matters. On one matter of fact, however, since it concerns others in addition to myself, may I beg the courtesy of a few words.

The first UNESCO Statement on Race was not, as your reviewer states, written largely by myself. It was written by the committee appointed to draft it. As *rapporteur* of the committee it fell to me to act as secretary. At the request of the committee I wrote the first draft, and after this was hammered into shape by the committee, I can by no stretch of the imagination conceive how I could be said to have been largely responsible for writing it. As for your re-

viewer's remark that the statement was so unacceptable that it had to be rewritten, the truth may be ascertained by any reader who cares to compare the first statement, mainly written by social scientists, with the second statement, mainly written by physical anthropologists and geneticists. The difference is as between Tweedledum and Tweedledee.

ASHLEY MONTAGU
321 Cherry Hill Road,
Princeton, New Jersey

I am sorry if I misrepresented Montagu's role in the preparation of the first UNESCO Statement on Race; I was reflecting what I feel to be the opinion of many physical anthropologists. Since it is only natural that he would be modest on this score, perhaps others connected with the project will set the record straight.

In the final sentence of his letter Montagu uses a literary reference to say that the second UNESCO Statement on Race differs only insignificantly from the first. This alleged equality is supposed to prove that the first statement was acceptable and did not need rewriting. Why then was it necessary to go to all the trouble of preparing a second statement? And why does Montagu take up space in his textbook with two "identical" statements? Here it is pertinent to point out that Comas, who was a member of the first committee, includes in his textbook *not* the first statement but the second. Why has he, a renowned physical anthropologist, abandoned his own committee's statement, unless he now feels that it is unacceptable?

T. D. STEWART
Department of Anthropology,
Smithsonian Institution,
Washington, D.C.

Authors as Indexers

John R. Clark's letter [*Science* **133**, 1040 (7 Apr. 1961)], suggesting that authors are best qualified to index their own books, misses the same point—of some moment for scientists and for scholarship as a whole—that is overlooked by the advocates of bibliographical machines.

Actually, of course, authors are seldom qualified to do indexing. Only occasionally can they do half as well as an experienced professional indexer. The fact that authors or publishers, or both, are frequently unwilling to pay a professional illustrates nicely their underestimation of the problems involved.

Aside from such general considerations as the special nature of indexing technique and the fact that some specialists cannot write intelligible prose without help, authors nearly always

have special slants or blind spots that make it difficult for them to do a good index. An individual may be the authority on the content of his particular book, but it does *not* follow that he has a good enough over-all, objective view of the whole field to which his book is a contribution to do a competent piece of indexing. This is particularly true in those fields where, for various reasons, terminology is not stabilized.

The point overlooked by both Clark and the gadgeteers is that research, writing, publishing, indexing, literature searching, and criticism are all parts of an extremely complex, ever-changing *social* process in which more judgments and decisions, involving values as well as subject-matter technicalities, must be made than any one person can handle.

HENRY BLACK
Bibliographical Services, New York, New York

Statistical Evidence

Warren Weaver's statement ["The disparagement of statistical evidence," *Science* **132**, 1859 (1960)] that "statistical evidence is, in essentially all nontrivial cases, the only sort of evidence we can possibly have" seems to me to be as "wholly unwarranted" as is the practice he very properly condemns—namely, the "automatic discarding of evidence because it is statistical." Statistical evidence is usually very useful, sometimes essential, but there certainly are many kinds of useful evidence that are not statistical in the usual meaning of that term. Our belief that the earth is not flat, that it revolves once in 24 hours, and that it completes an orbit around the sun in a year is not based on statistical evidence. Neither does our acceptance of the theory of evolution, or of a dozen other theories that might be mentioned, depend to any great degree on statistical evidence, although these theories may be and often are supported by such evidence.

The difficulty is partly semantic. Some writers seem to regard as statistical practically any method of dealing with quantitative data, but usually the term implies frequency distributions, standard errors, analysis of variance, correlation coefficients, and so on. Presumably it is these latter that Weaver had in mind. If so, he certainly must realize, on second thought, that failure to use these techniques does not automatically negate the usefulness of quantitative data. Mendel, for example, did not use them, and yet he revolutionized our ideas of heredity.

Weaver is not the first to imply or state that statistical methods are essen-

tial for all really worth-while research. And if we enlarge the subject to include mathematics, of which statistics is a branch, several noted scientists could be added to the list. Yet Darwin had little use for mathematics, and Faraday's experience with mathematics is said to have been limited to turning the crank of a calculating machine. G. N. Lewis has deplored the tendency of many to overrate mathematics.

Both mathematics and statistics have made, and will no doubt continue to make, revolutionary contributions. But we might well ask whether the occasional reluctance to use them or to accept the conclusion derived from them may not be due in part to the tendency to overemphasize their value. Pasteur pointed out many years ago that overemphasis of any kind leads to reaction, which, again overshooting the mark, makes the search for truth ever more difficult.

S. C. SALMON
4103 Roanoke Road,
Hyattsville, Maryland

Warren Weaver's editorial has stirred up some discussion here. As a statistician in experimental work, I should like to present a statistician's view.

The simple phrase *statistical evidence* is, unfortunately, widely used to describe two very different kinds of evidence: (*a*) that provided by data gathered from uncontrolled events just as they happened to occur "in life," and (*b*) that provided by data gathered from a planned experiment in which every effort is made to prevent the effects being studied from being confounded with effects of irrelevant factors.

Weaver mentions this dichotomy, but his ensuing remarks seem to imply that he regards the difference as one of degree. I would go further and say that the two are entirely different, and that a major reason for the slow improvement of the quality of scientific inference has been the confusion of *a* with *b*. Scientists who call both *a* and *b* "statistical evidence" have properly rejected *a* but have then gone on to reject *b* merely because they have given it the same name; the result is that they do not know or use as much statistics as they should. Some of them might even be surprised to know that the statisticians are very much on their side and have even invented a distinguishing phrase, "historical evidence," for evidence of the kind described under *a*.

Evidence of type *a* is exemplified by the sports column I once read in which the writer decried the firing of football coaches and produced "statistical evidence" of the folly of this action by displaying data showing that colleges which keep their coaches for long periods of time have much better records on the field. Much of the "statistical evidence" relating to smoking and lung cancer may be in a similar category; we do not know.

Type *a* evidence is good only for suggesting experiments which may produce type *b* evidence. Unfortunately, there are fields in which type *b* evidence cannot be obtained. This may force us to use type *a* evidence, but it does not force us to assign to it the degree of confidence that is associated with type *b*. In fact, those fields wherein type *b* evidence cannot be produced might be better described by some word other than *science*.

ROBERT HOOKE
Westinghouse Electric Corporation
Research Laboratories,
Pittsburgh, Pennsylvania

Warren Weaver's provocative comments on the power of statistical evidence merit repeating. They merit repeating in particular to scientists who are not mathematicians.

The disparagement of statistics on the part of the tobacco industry and of political parties, on which Weaver comments, is understandable (although not commendable) in view of the vested interests of these groups in the results. But the disparagement of statistics on the part of scientists—of men, presumably, with a vested interest only in seeking truth—is deplorable.

In a field of science, such as geology, where there are so many variables, the application of statistical methods is most appropriate. The usefulness of experimental design should be particularly obvious to any working geologist. Yet it is true that only the most prescient geologists are receptive not only to statistical evidence but to statistical methods as well.

I think the reasons for this reluctance are many. In part it reflects the general opposition people (and scientists among them) display toward new ideas that they cannot fully understand. Certainly this is true of geologists, who, as a group, know too little mathematics to comprehend even the general problem, and who are reluctant to accept new ideas that originate outside the ranks. There is also this factor: statistical methods have been taken up by a few zealots who, through a combination of ignorance and enthusiasm, have succeeded in overselling them to the scientific community. There is no doubt that the proper application of statistical theory requires a mathematical maturity not readily come by. Then, certainly, the wild use of statistics in the hands of the advertising agencies has not raised the reputation of statistical

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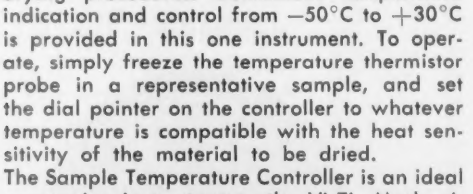

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methods with thoughtful segments of the public.

How to make the general scientific fraternity more receptive to these practical statistical methods poses a problem. It would seem to me that one effort which might help this situation would be to emphasize in education the importance of combining enough statistical theory with other scientific education to make scientists better informed with respect to its potential. And vice versa: urge the would-be statisticians to study enough of one other science so that they can comprehend the problem to which they may subsequently apply their methods. Perhaps, also, there should be more general articles written by qualified statistical experts, who could write for the essentially nonmathematical scientist in *his* technical journals. These articles should describe specific case studies where statistical methods apply.

In any event, I greet Weaver's editorial as welcome prose in *Science*.

M. MATHEZ
Standard Oil Company (New Jersey), New York, New York

Warren Weaver's editorial is trenchant and timely and deals effectively with a most urgent problem. In directing attention to the growing role of probabilistic processes, Weaver has done all of us a great service.

In my opinion, the effectiveness of Weaver's message is marred by an unfortunate usage of the phrase *statistical evidence*. It is important to distinguish between evidence from both experimental and nonexperimental observations, and between the statistical methods with which this evidence is analyzed. For example, the relationship between cigarette smoking and lung cancer, on the basis of all evidence and its analysis, is amply confirmed. The basis for this conviction, however, is not so much statistical as it is the convergence of multiple studies to support the same conclusions, and the finding that diminution in cigarette smoking leads to diminished risk of lung cancer and other diseases. The nub of this protest is that the "evidence" ought to be described by an adjective referring to the nature of the problem and to methods of collecting data—for example, genetic evidence, mortality evidence, and radiological evidence. The proper adjectival use of the word *statistical* seems to me to be restricted to the method by which the data are analyzed and to conclusions drawn concerning whether or not the data conform to random distributions or to other prescribed distributions.

This objection would be trivial were it not for the suspicion that the use of the phrase *statistical evidence* tends to obscure a more fundamental property of many of the sets of evidence which are cited in the editorial—namely, that the evidence is obtained from the observation of naturally occurring phenomena rather than from experiments. It is precisely in this realm of evidence that statistical analysis has provided some powerful insights; nevertheless, the nature of the evidence is often of greater importance to the interpretation of any conclusions than the fact that it was subjected to statistical analysis. As indicated by three out of the four examples given in the editorial, there is special likelihood that man or his reactions will be studied from sets of nonexperimental rather than experimental observations. It is a matter of some concern that, by placing the emphasis on statistical analysis or even upon the probabilistic character of data, the inherent problems associated with the collection and proper interpretation of nonexperimental data about man may readily be overlooked.

These problems are particularly serious in areas of chronic-disease epidemiology, in sociology, and in human genetics. One of these inherent problems is the recognition, measurement, and selection for study of important variables which may influence the as-

sociation which is being studied. A second problem is the collection of data under circumstances which prevent bias and which provide full freedom for the data collected to refute the hypothesis being tested. A third problem has to do with the relationship of evidence to conclusions. While statistical criteria are often important, the convergence of evidence from several independent studies, the demonstration of joint gradients in the magnitude of the supposedly related variables, and the demonstration of reversibility of the dependent variable by the selection and study of suitable sets of data are important guides for drawing conclusions from nonexperimental data. Despite the important contributions which statisticians have made to the management of the inherent problems of nonexperimental data, these are not solely statistical problems but problems of logic and method.

That there are other areas of science where comparable problems exist is shown in the same issue of *Science* by the article "Recent statistical studies in astronomy" [**132**, 1870 (1960)]. Here, also, the basic data are nonexperimental, but the use of systematic data collection and skilled statistical analysis permits some striking and very important conclusions. It is of interest that the use of statistics in the article about astronomy has not led the author to describe his data as "statistical" data. It is proper to describe them as "astronomical" data.

The word *evidence* is also used to designate certain facts presented in a trial; following this analogy, one thinks of statistics as a sort of jury or judge, which helps to decide, on the basis of law (accepted scientific criteria), whether the evidence presented supports the allegations of counsel (the scientists) with sufficient certainty to lead to a verdict. With naturally occurring data, particularly concerning man, we need to focus more attention on the adequacy of the criteria for drawing conclusions (the law, in the analogy of the trial).

In my opinion, use of the phrase *statistical evidence* should be discouraged, since it tends to obscure recognition of this problem.

JOHN R. GOLDSMITH
State of California Department of Public Health, Berkeley

I am delighted that my editorial aroused some interest, and I appreciate a chance to comment. I will not deal in detail with all four letters, for that would be tedious and repetitive. I think a few remarks may clarify my position.

1) An editorial in *Science* contains about 500 words. It is not feasible very fully to develop a subject—even a restricted and minor one—in so brief a statement, nor is there space to give qualifying refinements.

2) The phrase *statistical evidence* appears to have been assumed synonymous with statements made by professional statisticians and making explicit use of "frequency distributions, . . . analysis of variance, correlation coefficients, and so on." There even was some assumption (in other letters which I received) that I had argued in favor of *all* "statistical" reasoning, whether good or bad!

Not being a statistician, I used the phrase less professionally and much more broadly. By statistical evidence I meant evidence in the gathering *or* analysis of which probabilistic considerations enter. Since the entire universe is, as far as science now knows, made up of elementary particles all of whose ultimate laws are probabilistic in nature, it would, at least to me, seem rather difficult to produce any evidence which does not, at some stage of refinement, involve such considerations.

If Salmon will approach the subject from this point of view, he will, I am sure, recognize why I think his "nonstatistical" conclusions (about the nonflatness of the earth, and so on) are so inescapably statistical. His remark that

the earth "revolves once in 24 hours" is particularly vulnerable, partly because it is strictly untrue, but chiefly because one cannot possibly discuss the degree of accuracy of the statement or the reliability of the evidence without a detailed and explicitly statistical analysis.

3) One can, of course do a great deal of very useful and quantitative science without utilizing the procedures of the professional statistician, and certainly without bothering to remember that all evidence is (in the sense stated above) statistical. I had no slightest intention of implying otherwise.

4) Hooke states that the phrase *statistical evidence* is applied to data gathered by observation of uncontrolled events, and also to data gathered from planned experiments. Since these two together include, as far as I can see, all data, Hooke would appear to agree with me that all evidence is statistical evidence. But Hooke has a low opinion of type *a* evidence, this naturally resulting from the fact that by his type *a* he really means (as revealed by his next-to-last paragraph) badly argued conclusions from poorly observed data. Darwin used type *a* evidence and revolutionized man's thinking in the process.

5) I do not advocate turning all of science over to the statisticians, nor do I think that a small boy, when he is counting his marbles, need be reminded that the counting of electrons is a very queer and slippery business. But I do object to the snide implication that evidence which is "merely statistical" is, by virtue of that fact, silly and unreliable.

WARREN WEAVER
Alfred P. Sloan Foundation, New York, New York

Nature and Nurture

Questions about the effect of environment on intellectual potential [G. Allen, *Science* **133**, 378 (1961)] should be considered in light of Spitz's studies on "hospitalism" [R. A. Spitz, in *Psychoanalytic Study of the Child* (International Universities Press, New York, 1946), vol. 1, pp. 53–74]. Controlled studies of institutionalized infants showed a drop in developmental quotient from 124 to 72 in institution X during a given period; in institution Y there was no change, and infants at home in comparable socioeconomic areas showed no change. The significant variable was the presence of one mother or mother substitute for each infant in institution Y and of one mother-substitute for each eight infants in institution X.

Follow-up studies 2 years later

[R. A. Spitz, *ibid.* (1947), vol. 2, pp. 113–117] showed profound emotional and intellectual crippling among the surviving infants from the original study in institution X. In addition, in height and weight they were considerably below the expected levels for their age. I urge those interested in this subject to read Spitz's carefully documented and lucid report.

PETER D. KING
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Fallout

In their article "Atmospheric transport of artificial radioactivity," Martell and Drevinsky (*1*) undertake to demonstrate that the yield, for a given size of stratospheric source, from the Russian weapon test at about latitude 52°N in the autumn of 1955 was greater than that from the American equatorial tests (at 11°N), Castle in 1954 and Redwing in 1956, by factors of 60 and 10, respectively. Martell and Drevinsky calculated these factors from Sr^{90} and Sr^{89} data (*2*) from the rain-collecting station at Milford Haven, Wales, and from estimates of the stratospheric sources by Libby (*3*). We offer here an alternative interpretation of the same data and suggest that the relative yield of the high-latitude test has been overestimated.

Peirson *et al.* (*4*) calculated that not more than 13 percent of the Sr^{90} collected in rain at Milford Haven during the spring of 1956 was due to the Russian tests of 1955. The effect of the Russian tests was seen against a background of stratospheric debris from the 1954 Castle series. This percentage is derived from the values for the ratio Sr^{89}/Sr^{90} after correction for the radioactive decay to 22 November 1955, the date of the only high-yield test of this Russian series (*5*).

The amount of radioactivity injected into the stratosphere by these weapon tests was estimated by Libby (*3*) to be 20 "megatons of fission" for Castle and 1.8 megatons for the Russian 1955 series. Then, on the basis of Martell and Drevinsky's parameter (micromicrocuries of Sr^{90} per liter per megaton), the ratio of yields during the first half of 1956 is

$$\frac{\text{Russian 1955}}{\text{Castle 1954}} = \frac{13}{87} \times \frac{20}{1.8} = 1.7$$

The estimate of 13 percent for the Russian contribution is a subjective estimate of the upper limit, since a significant proportion of the *new* debris during this period could well have been of tropospheric origin. If, however, all the new debris is attributed

to the Russian 1955 test, then the proportion would be raised to 22 percent, and the ratio of yields, to 3.2. On the basis of this interpretation, Martell and Drevinsky's estimate is 20 to 30 times too high.

Where our approach differs from the foregoing interpretation is in the choice of (i) the appropriate reference date for the only reported high-yield explosion for the Russian test series of the autumn of 1955 and (ii) a period in which the Russian debris can be compared with contemporary Castle debris. A comparison in the same period avoids the error of the foregoing interpretation, caused by discounting the effect of the seasonal variation in Sr^{90} activity.

The relative yield from the Redwing 1956 test series may be derived in a similar manner. It has been estimated (*4*) that 78 percent of the Sr^{90} collected at Milford Haven during the autumn of 1956 could be attributed to Redwing. (For simplicity of analysis, the Redwing debris is considered in relation to a background consisting essentially of Castle debris, and the fraction due to the Russian 1955 tests is ignored.) On the basis of Libby's estimates (*3*) of the stratospheric source strengths, the ratio of yields during the autumn of 1956 is found to be

$$\frac{\text{Redwing 1956}}{\text{Castle 1954}} \approx \frac{78}{22} \times \frac{20}{6.7} \approx 11$$

This ratio is about twice that calculated by Martell and Drevinsky, who in this case have overestimated the contribution of Sr^{90} from Castle by selecting an inappropriate period of measurement.

The relevance of this type of calculation can be no greater than that of the stratospheric injection data. As Martell and Drevinsky suggest, the stratospheric component from these weapon tests is uncertain. Also, it is improbable that the selected data, from a single measuring station, would provide a truly comprehensive index of comparison for the relative global yields of these weapon tests.

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It is surprising that two interpretations of the same aspect of fallout can vary so widely. The Peirson and Stewart analysis gives a relative rate of stratospheric fallout from the 1955 Soviet tests and the 1954 Castle tests which differs from our result by a factor of 20 to 30. Part of the difference is due to a change in the time basis of comparison. The physical consequences of the Peirson-Stewart interpretation (discussed below) suggest that these authors have seriously underestimated the contribution of the 1955 Soviet tests to fallout during the first half of 1956. In every respect in which the assumptions and method of Peirson and Stewart differ from our own, they tend to reduce the contribution of the 1955 Soviet tests relative to that of the Castle tests. Since our interpretation of the Milford Haven rainfall data has been presented elsewhere (*1*, *2*), we address our attention to the several points of disagreement.

In our analysis of the Milford Haven data (*2*, Fig. 2), we compared the relative intensities of stratospheric fallout for the Castle, Redwing, and 1955 Soviet tests at corresponding early times after test injection. Since we have concluded from our Ba^{140}/Sr^{90} data (*2*) that most short-lived fission products in world-wide fallout are of stratospheric origin, the initial large differences in fallout rate acquire special significance. Furthermore, during these early periods the contribution of each test source can be assessed unequivocally from Sr^{89}/Sr^{90} data within the stated uncertainties of production ratio and production date. By contrast, Peirson and Stewart attempt to resolve the concurrent contributions of the 1955 Soviet and Castle tests during the first half of 1956 and of Castle and Redwing during the autumn of 1956, thus employing not only a different time scale of comparison but a far more subjective procedure. Any uncertainty in the estimation of one component affects the other component in the opposite sense, magnifying the uncertainty in the ratio. Peirson and Stewart's assignment of all unidentified Sr^{90} to Castle is a dubious procedure, particularly for the autumn of 1956, a period for which residual stratospheric debris from the 1955 Soviet tests cannot be ruled out.

The Peirson and Stewart analysis results in assignment to the Castle tests of 87 percent of the Sr^{90} fallout in the first half of 1956 and 22 percent of that in the autumn of 1956. Applying these percentages to the Milford Haven rainfall data (*3*) gives values for Castle components of 5.6 $\mu\mu$c/lit. for the pe-

riod 14 February to 9 July 1956 and of only 0.52 $\mu\mu$c/lit. for the period 2 August to 1 December 1956. Seasonal variation of this magnitude, with levels varying by a factor of nearly 11 during 1956, is a surprising result for Castle debris, which had been injected into the high equatorial stratosphere more than 2 years earlier. This remarkable variation is all the greater when only the spring peak and the fall minimum periods are compared, and also when the Castle component in rainfall in the autumn of 1956 is corrected for residual debris from the 1955 Soviet tests. By contrast, Stewart (*3*) has shown that levels of Sr^{90} in rains at Ohakea (latitude 40°12'S) varied seasonally by less than a factor of 2 during 1956, when Castle was unquestionably the only significant source of fallout in the Southern Hemisphere. Observed seasonal variations in ozone levels in tropospheric air in the Northern Hemisphere are similarly small. We suggest that the remarkable seasonal variation in Sr^{90} fallout from Castle which results from the Peirson-Stewart analysis is not real and is due to underestimation of the contribution of the 1955 Soviet tests during the first half of 1956.

Peirson's method (*4*) of estimating the Soviet-test component in fallout of Sr^{90} during the first half of 1956 differs from our own in two important respects, each of which results in an underestimation on Peirson's part, or an overestimation on ours, of the Soviet test contribution. First, Peirson arbitrarily assigns a substantial fraction of the Sr^{89}, and thus some of the Sr^{90}, to tropospheric sources. On the basis of the Ba^{140}/Sr^{90} data for New England rains (*2*) we have concluded that substantially all Sr^{89} in world-wide fallout is stratospheric in origin. The Milford Haven Sr^{89}/Sr^{90} data for the first half of 1956 are consistent with assignment of all Sr^{89} to the 1955 Soviet tests. Second, Peirson takes the high-yield 23 November shot alone as the source of stratospheric fallout from the 1955 Soviet tests. Other shots in that series took place on 4 August, 24 September, and 10 November. Although the yield and the cloud heights for these events have not been made public, they cannot be excluded from consideration. Surface shots of 100 kilotons and air shots of even lower yield would inject debris into the lower stratosphere at latitudes of Soviet testing (*5*). In our own analysis we assume stratospheric origin of the Sr^{89} and a Sr^{89}/Sr^{90} activity-production ratio of 170. For the Milford Haven data for the first half of 1956, this leads to an assignment to the stratosphere of 25 percent of the Sr^{90} fallout for the 23 November 1955 shot alone, or nearly 100 percent of the fallout for the 4 August 1955 shot

alone. We take the mid-point of the 1955 Soviet test series as the production date for which it can be assumed, with a factor of uncertainty of 2, that 50 percent of the debris was of 1955 Soviet test origin.

On this basis, we suggest that Peirson and Stewart have underestimated the 1955 Soviet contribution by a factor of between 2 and 8, the Soviet-Castle ratio being thus affected by a much larger factor. The physical consequences of their interpretation indicate that the higher factors must apply. The remaining difference can be explained on the basis of differences in rate of deposition of Sr^{90} fallout from Castle for the two quite different periods considered.

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On Reading Original Papers

The light-hearted editorial, "Electricity and personal magnetism," in your issue of 3 March [*Science* **133**, 611 (1961)] makes amusing reading, but it exhibits the lack of understanding that is at the root of C. P. Snow's "Two cultures." While I cannot claim to have read all 2.5 million words of the "Great Books of the Western World," or even the 642 pages of Faraday's *Experimental Researches in Electricity* (and am in no way connected with the publishers or endorsers), I am sure that your editorial view of what constitutes good reading about science is an extremely limited one.

As I understand it, the writer of the editorial proposes that a reader be told what parts of a scientific work are "really great," what terms are to be considered "right," and where a scientist of the caliber of Galileo, Newton, Faraday, or Darwin was "wrong." Apparently he feels that it is a waste of time to "make one's way" through lengthy, outdated material in the classic works of science when the confirmed results can be condensed to half a page

in a modern textbook. And many of yesterday's scientists agree with him. From his point of view, all that counts in science are the currently accepted *results*.

Of course, most of your readers, including myself, *are* interested in results, but we would be blind indeed if we thought there were nothing else of importance in science but results. The scientific mode of thought, through which these results can be said to have been arrived at, is relatively recent in the history of mankind and is far from being accepted by the majority today —even by the majority of intellectuals. The acceptance of scientific results is easy—too easy—and has led to a common view of science very similar to that of magic in primitive cultures (see, for example, Malinowski's *Magic, Science, and Religion*). Most educated people "know," for instance, that matter is composed of atoms, but how many of them have a clear idea of the complex reasoning from necessarily incomplete and confused data that led to this concept? When the incompleteness and confusion are eliminated in a textbook, this isn't presumptuous so much as simply misleading. How can such treatment give any insight into the development of science or the turbulent frontiers of science today?

I dare say many criticisms can be made of the "Great Books of the Western World," but the criticism leveled in the editorial is not the one to make if you are interested in better understanding of science on the part of intelligent nonscientists.

THORNTON PAGE

Wesleyan University,
Middletown, Connecticut

Your editorial concerning the "Great Books of the Western World" completely misses the point. Editors of *Science* cannot be blamed, perhaps, for being unfamiliar with the wacky world of sales promotion and "consumer motivation," and with the workings of the unscientific mind.

The truth is that most of the people who buy the "Great Books" have no intention of actually reading the material. The set is a prestige object, an exhibit for guests, filling for a handsome bookcase, or a source of pride for a booklover.

I know a person who, having just installed a set of bookshelves, went to a second-hand bookstore and bought a box full of books selected at random for their covers. Buying a set of the "Great Books" is a more sophisticated way of doing the same thing. At the other end of the scale, there are empty ornamental book covers on the market for the economy-minded.

I'm serious!

KIRBY WALKER

609 Wendy Lane,
New Orleans, Louisiana

Your editorial points up most effectively the weakness of the "Great Books" curriculum. The advocates of such an educational sequence seem to imply that to be educated one must repeat the experience of the race, at least as represented in the writing of the great minds of history. But the true curriculum has to be a *short cut* to the experience of the race. And that conception does not preclude all firsthand contact with the writings of bygone centuries; it means simply that we must be sufficiently selective so that within practical time limits we may help the learner to gain understanding and control of his present environment.

P. W. HUTSON

University of Pittsburgh,
Pittsburgh, Pennsylvania

I agree completely with Thornton Page that there is great value in reading original scientific papers. The point of my editorial, however, was that the approach followed in the "Great Books" befuddles the general reader unnecessarily, and hence should not be

endorsed. There are also unnecessary opportunities for befuddlement in some of the humanities selections. Let me cite an example from the Jowett translation of Plato's *Republic*, which dates from the Victorian era and which is the translation offered by the "Great Books." As noted by Cornford in the preface to his own more recent translation, the reader of Jowett, when he lights on "the statement . . . that the best guardian for a man's 'virtue' is 'philosophy tempered with music,' might run away with the idea that, in order to avoid irregular relations with women, he had better play the violin in the intervals of studying metaphysics." Not only is this idea false, as the violinist in the Tabu ad has learned to his peril, but this is not what Plato meant by describing (again to quote Cornford) "*logos*, combined with *musiké*, as the only sure safeguard of *areté*."—J.T.

Economics in the News

The tenor of your staff reporter's observations on the economic philosophies supposedly animating the Eisenhower and Kennedy Administrations [*Science* **133**, 367 (1961)] prompts me to register an objection and to offer a constructive proposal.

As a political independent who participated in the preparation of all eight of President Eisenhower's annual economic reports to Congress, I find this piece both superficial and intellectually offensive. To identify the Eisenhower position with "the dismal science" while characterizing "Kennedy's economics" as "the dismal science made cheery" may be good enough journalism and may be assumed to be consistent with the emotional commitment of a substantial fraction of the scientific community. But the Carlylean allusion is anachronistic, certainly since the passage of the Employment Act of 1946 with strong bipartisan support; and the equally Carlylean hero worship manifested by your reporter is inappropriate, not only in a scientific publication but also in a pluralistic democracy in which the economic roles of the President and of the federal government altogether are deliberately confined and in which the "declaration of policy" inserted into an employment act must be so burdened with qualification that it cannot provide an unambiguous standard for administration.

As a member (fellow) of the AAAS, I suggest that the same kind of criteria of objectivity, reliability, and high seriousness that presumably apply to the section of *Science* devoted to research reports be extended to contributions to "Science in the News."

Surely, the association has a much greater stake in teaching than in delighting. It can best upgrade the average level of literacy of its readers regarding other people's specialties by providing unbiased, straightforward, factual reports and balanced analyses. Above all, it must avoid the appearance of concurring in the bizarre proposition that the star of scientific advancement should be hitched to a particular political wagon.

IRVING H. SIEGEL
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Effects of Punishment on Behavior

In his report entitled "Punishment in the squirrel monkey *Saimiri sciurea*" (*1*), James B. Appel states "little is known about the effects of punishment on the behavior of higher organisms." He presents his observations in the evident belief that long-lasting effects have not previously been reported.

Evidence has for many years been available that punishment by electric shock can condition extremely persistent anxiety reactions to previously "neutral" stimuli, accompanied by inhibition of other responses in the presence of the conditioned stimuli, just as Appel observed in his monkeys. The existence of this evidence may have been obscured by its coming under the heading "experimental neurosis." The first reports, as usual, came from Pavlov's laboratories, where dogs were the subjects; subsequently, similar effects were reported in cats by Dimmick, Ludlow, and Whiteman (*2*) and by Masserman (*3*). The Russian workers believed that the persistent behavioral changes in their animals were the result of a "clash" between excitation and inhibition, and Dimmick *et al.* and Masserman attributed the changes in theirs to conflict between feeding and avoidance motivations, because all their animals were punished in circumstances in which food-approach behavior had been conditioned. However, a few years later I demonstrated (*4*) that in cats subjected, without any preliminary training with food in the experimental cage, to high-voltage, low-amperage shocks in the cage situation, the same persistent behavior, characterized by marked anxiety responses, is conditioned. Thus, despite the opinions of the earlier experimenters, the effects reported by them must be regarded as a straightforward conditioning of emotional and other reactions primarily evoked by electric shock.

Though these conditioned autonomic reactions do not disappear through

the ordinary process of extinction, it is relatively easy to eliminate them by counterposing other emotional responses that are antagonistic to the anxiety in the sense that their evocation is accompanied by reciprocal inhibition of anxiety responses (*4*). The extension of this principle to persistent unadaptive habits (neuroses) in human subjects has led to significant new methods of therapy (*5*).

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Wolpe's letter points toward two problems which have existed in the behavioral sciences for many years. The first concerns the categorization, communication, and (perhaps most unforunately) selection of evidence.

While the studies Wolpe mentions, and many others besides, such as those of Solomon and Wynne on "traumatic avoidance" (*1*), indicate that "persistent anxiety reactions" may develop after exposure to electric shock, there is an equally imposing number of investigations which seem to suggest that shock has generally temporary effects or, at best, may facilitate the acquisition of responses which are incompatible with that for which the subject is being punished. I chose to limit my brief remarks to the experiments of Estes (*2*) and Azrin (*3*), because the experimental procedures of these authors closely resembled my own. Moreover, I was interested, together with Estes and Azrin, in *punishment* rather than conflict, anxiety, or experimental neurosis.

It is unfortunate that similar or even identical bits of information are often overlooked, as such, or are confused in a mass of professional jargon even within the field of experimental psychology. This is, I believe, a result of the many different conceptualizations investigators impose upon their data when reporting their results. The confusion would be attenuated if events were described objectively in terms of the variables manipulated. It is obvious that a statement about the effects of electric shock upon the rate of response on a variable-interval schedule of reinforcement is much clearer than a similar account of the effects of conflict on the production of anxiety states.

A second and far more important problem arises indirectly from Wolpe's

comments. If it is true that contradictory conclusions have been drawn from the results of experiments concerned with the effects of electric shock, one might wonder why enough systematic studies have not been made to relate this variability to experimental parameters such as species, shock intensity, and previous history of the animal. Such a program has been largely neglected in favor of short, exciting theoretical studies which may be reinforcing to the experimenter but which seem to have added little to the store of communal knowledge about one of the great problems in contemporary behavioral science, that of aversive control.

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Quantum Mechanics and Biology

The molecular basis of energy transduction in biological systems has been excellently reviewed by Green and Hatefi *(1)*. However, quantum mechanical contributions make it possible to relate these energy changes to more fundamental submolecular processes. Indeed, the application of quantum theory offers the most promising approach to as yet unanswered problems. This viewpoint has been eloquently expressed by Szent-Györgyi *(2)*.

Green and Hatefi *(1)* quite correctly conclude that electron flow in transport systems is not a result of simple molecular collision. Quantum mechanics is more specific. The rapidity of electron flow stems from the redistribution—a practically instantaneous process—of molecular orbital energies. Energy transduction based on such redistributions need not contemplate any appreciable linear movement of the electrons. The quantum-mechanically derived π-electron system fully satisfies the requirements for a catalytically functioning mechanism. This is particularly true for the unsaturated conjugated structures in which the electrons are coupled resonators. This completely allows for both instantaneous and distant site transfer of energy. The known facts concerning biologically active compounds (purines and pyrimidines) are in accord with these concepts. They are predominantly unsaturated, conjugated structures.

As pointed out by Green and Hatefi *(1)*, the mitochondrial lipids are char-

acterized by fatty acid residues with a high degree of unsaturation. This unsaturation is quite consistent with the ability of lipids to provide a high degree of stereospecificity combined with participation in the π-electron system of associated proteins. Thus, the role of lipid in electron transport instead of being "far from clear," as indicated by these authors, is, on the contrary, a very distinct one when based on quantum-mechanical considerations.

The respiratory coenzymes have been shown by Green and Hatefi *(1)* to play a predominant role in electron transport. The redox mechanism of these coenzymes has also been specifically related to the molecular orbital energies of their π electrons. Employing LCAO (linear combination of atomic orbitals) calculations, Pullman and Pullman *(3)* have obtained resonance integral coefficients for the enzymes which explain their redox function. The coefficient for the homo (highest occupied molecular orbital) of reduced flavin mononucleotide ($FMNH_2$) turns out to be negative. Since this is quantum-mechanically indicative of antibonding character, the finding is in remarkable accord with the known fact that this compound is autoxidizable. While similar calculations have not as yet been reported for the cytochrome components of the transport system, evidence is not lacking on which to predict that some will be found with negative homo coefficients. Dixon *et al. (4)* have reported on an irreversible autoxidation of cytochrome *c* reductase. Armstrong *et al. (5)* have reported a similar autoxidation of cytochrome b_2 lactate dehydrogenase. Both autoxidations involve flavin dissociation.

The findings reported above are not without potential application to medical problems. Dehydrogenase deficiencies have been implicated in genetically transmitted and sex-linked transmitted diseases (see *6*). Theoretical quantum mechanical calculations involving π electrons have also been applied to the study of carcinogenesis (see *7*). The ability to theoretically predetermine those particular components of an enzyme system which may be the ultimate source of a distorted metabolism offers exciting frontiers for the biological scientist. A great deal of ground work has been done by the physicist, and if the quantum guideposts are read correctly, this ability could be within the realm of realization.

No doubt, as Szent-Györgyi (*2*) has emphasized, the mastering of a new discipline like quantum mechanics is a Herculean task for the biologist, but it is a road that must be traveled. Nor is it necessarily a one-way street. It is of interest to note the increasing application of biological phenomena to the solution of electronic and engineering problems. Simulation of neurons, memory tracks, and retinas has become the basis of the entirely new field of bionics (see *8*). It is not unlikely that new and revolutionary concepts for physics will have their origins in biology. Knowledge gained in the study of photosynthetic and bioluminescent systems is today providing answers to questions concerned with the storage of solar energy. Such answers may, in the future, provide the means of sustaining life in outer space. All of which points up the basic unity and universality of the various disciplines of science.

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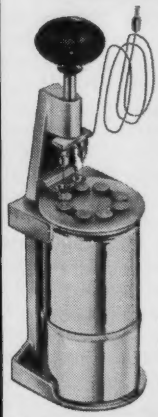

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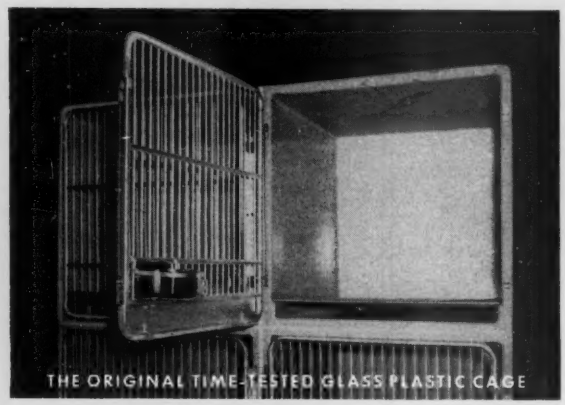

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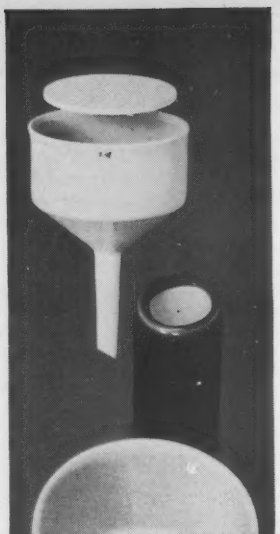

Size and Productivity

Harmon's very interesting analysis of "High school backgrounds of science doctorates" [*Science* **133**, 679 (1961)] will surely be widely studied and quoted. One aspect invites immediate comment.

The correlation between the size of the graduating class and the productivity of doctorates in all fields was demonstrated unequivocally. However, the implication that large size is *responsible* for high productivity has been reflected in newspaper headlines based on this article. Surely the size of the graduating class is related to the population density of the area and therefore to at least two other important variables, the economic status of the parents and the cultural backgrounds of the families. There was no attempt in Harmon's study to equate such variables; indeed, this would require many more data from the schools and would even then be very difficult. But it seems wrong to assume that by combining small units into large ones these wide differences will disappear.

To give an extreme example, high schools with graduating class size "greater than 800" produced twice as many Ph.D. students as those of class size "600–800," and a simple first analysis would lead us to change all to the very large units. But the "600–800" unit is surely sufficiently large for purposes of organization to best serve the needs of talented students.

It is even difficult to tell from the results presented whether the striking variations of productivity with geographical area and with size are in fact independent, or whether these simply are the result of a predominance of large high schools in the New England and Middle Atlantic states and of small ones in the East South Central area.

Conant and others have argued on logical grounds that adequate size is a major requirement if a high school is to offer a good program. This is certainly confirmed by the experience of those who have tried to provide an enriched program in a small high school. But the statistics which support this conclusion must be analyzed and interpreted with care, lest the problem of providing a good education appear simpler than it really is.

STANLEY C. BUNCE
Department of Chemistry, Rensselaer Polytechnic Institute, Troy, New York

Harmon's analysis of the high school backgrounds of Ph.D.'s may indeed substantiate the suspicion of many that individuals with degrees in education are recruited from the lowest I.Q. level, but biologists can take little comfort from the finding that their field is populated by students of the next lowest

I.Q. level. It is not entirely correct, however, to make the sweeping inference that this may be due to a failure in presentation (p. 684). It has long been the custom for high school counselors to advise the most promising students to take physics and chemistry in preference to biology in high school as part of their college preparation, and also to encourage the less able students to take biology as a science. There is much justification for this practice, since college courses in physics and chemistry are required for the biology major as well, and to have had the preliminary high school course does place the student at an advantage in the more rigorous and competitive college courses. From this, many students have drawn the inference that biology is a second-rate subject. The result has been, in many schools, a rule-of-thumb selection at the start, and it seems probable that many high school students whose original interest in science was in the field of biology have been diverted into physical sciences.

Therefore it is regrettable that, in making his study, Harmon did not also obtain data on counseling practices. No one will deny that the high school biology curriculum needs improving, and concerted effort is being made toward this end by the American Institute of Biological Sciences, but it seems obvious that if biology is to recruit its share of the best students, some changes in the attitude of counselors will also be necessary.

JOEL W. HEDGPETH
Pacific Marine Station

Productivity of Ph.D.-bound graduates is indeed a compound of many factors, acting together rather than independently, as Bunce says. Some of the very large schools, in New York City especially, are boys' schools exclusively and also impose aptitude requirements for entrance. The effects are thus confounded. We hope to make further studies which can at least partially control both individual and community factors of importance in this connection.

As for Hedgpeth's comment, no doubt poor counseling by both teachers and counselors contributed to a vicious cycle that produced the situation we observe today. Unfortunately, the counseling practices prevalent in the high schools 15 to 20 years ago cannot be accurately determined now and certainly cannot be measured by reference to selected high school transcripts. The remedy of more rigorous and challenging courses, taught by more capable teachers, would no doubt quickly reorient both counselors and students.

LINDSEY R. HARMON
National Academy of Sciences–National Research Council

INDEX OF ADVERTISERS—19 May 1961

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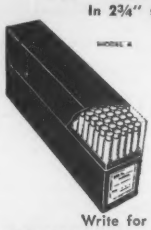

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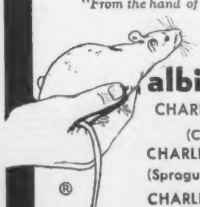

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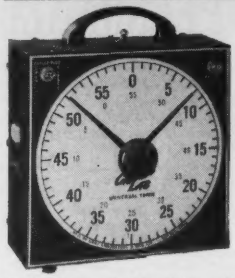

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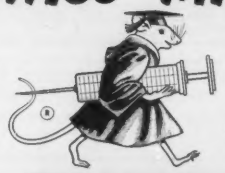

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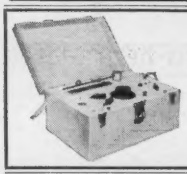

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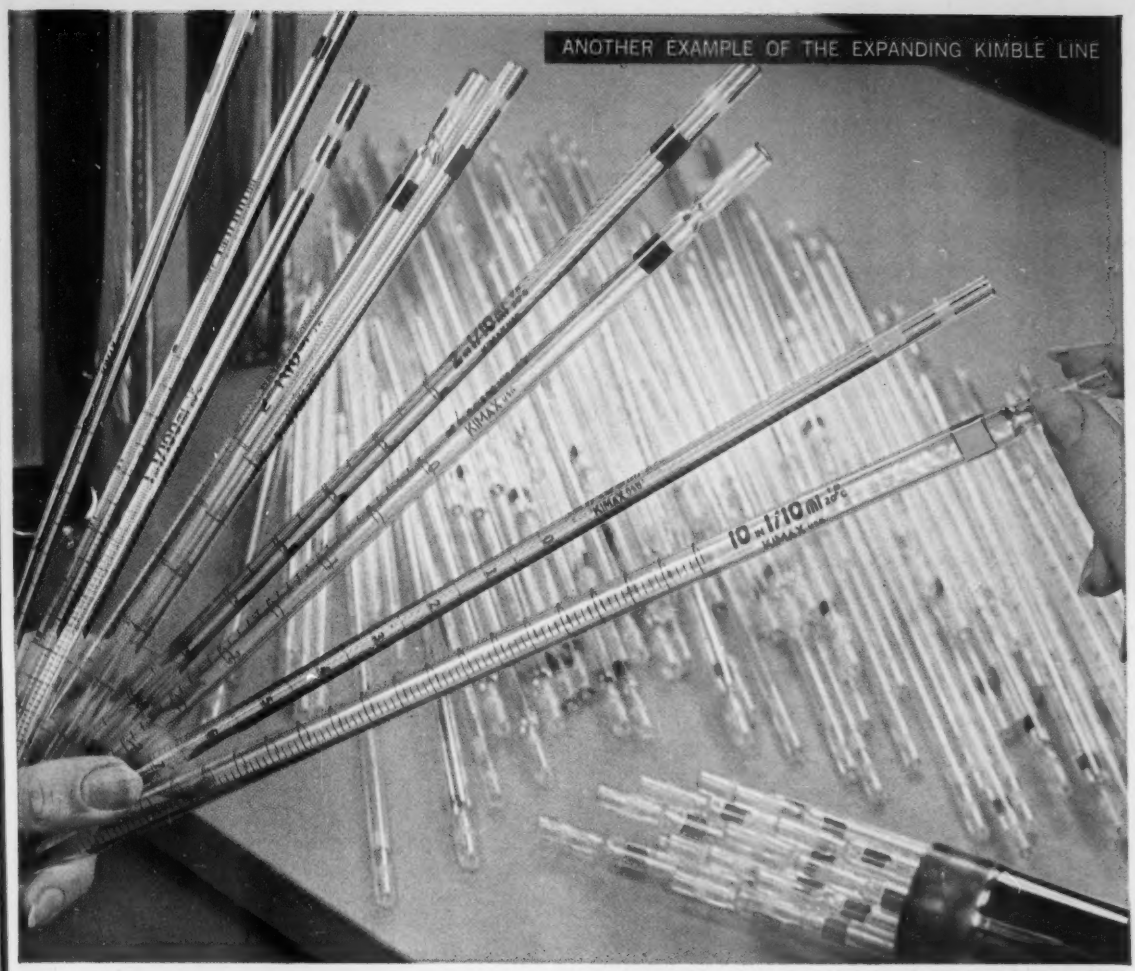

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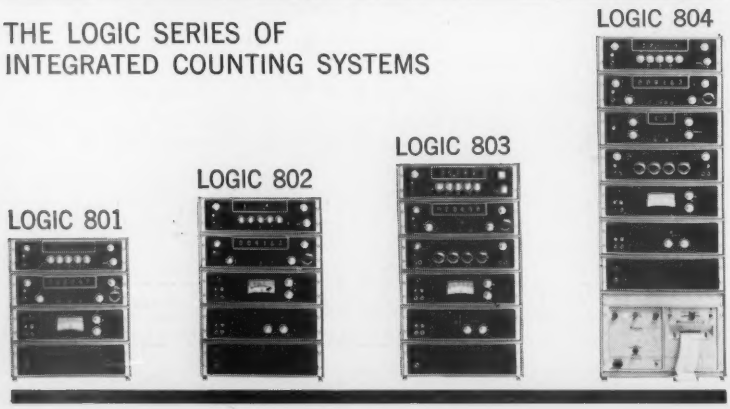

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